THE CENTURY SCIENCE SERIES

EDITED BY SIR HENRY E. ROSCOE, D.C.L., LL.D., F.R.S.

JOHN DALTON

AND THE

RISE OF MODERN CHEMISTRY

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INTRODUCTION.

In the vestibule of the Manchester Town Hall are placed two life-sized marble statues facing each other. One of these is that of John Dalton, by Chantrey; the other that of James Prescott Joule, by Gilbert. Thus honour is done to Manchester's two greatest sons — to Dalton, the founder of modern Chemistry and of the Atomic Theory, and the discoverer of the laws of chemical-combining proportions; to Joule, the founder of modern Physics and the discoverer of the law of the Conservation of Energy. The one gave to the world the final and satisfactory proof of the great principle, long surmised and often dwelt upon, that in every kind of chemical change no loss of matter occurs; the other proved that in all the varied modes of physical change no loss of energy takes place. Dalton, by determining the relative weights of the atoms which

take part in chemical change, proved that every such change — whether from visible to invisible, from solid to liquid, or from liquid to gas — can be represented quantitatively by a chemical equation; and he created the Atomic Theory of Chemistry by which these changes are explained. Joule, by exact experiment, proved the truth of the same statement for the different forms of energy.

As we can neither create nor destroy matter, so also we can neither create nor destroy energy. As when the candle burns and the wax disappears, its constituent parts are not lost, but escape in the form of steam and carbonic acid gas formed by the union of the hydrogen and carbon of the wax with atmospheric oxygen; so the energy of the chemical forces locked up, or potential, in the wax and oxygen becomes evident, or kinetic, in the heat of the flame. In other words, the molecular motion of the particles becomes motion of the mass. And just as there is a defiuite and unalterable relation of weight between the carbon and the hydrogen of the wax and the products of their combustion — carbonic acid and water — so there is a definite and unalterable relation between the amount of the chemical

potential energy of the constituents of the wax, and that of the heat evolved by their oxidation. Each relation can be expressed by numbers, and these numbers are the Foundation Constants of science. By the determination of the combining weights of the elements, and of their compounds, Dalton ascertained the truth of the first principle. By rigorous experiment, Joule measured the mechanical equivalent of heat, and proved that a weight of 772 lbs. falling through the space of one foot developed a fixed and unalterable amount of heat capable of raising the temperature of one pound of water one degree Fahrenheit — thus proving the truth of the second.

Before John Dalton's discovery of the laws of chemical combination, and without his Atomic Theory to explain those laws, Chemistry as an exact science did not exist, because although many facts were known, the relation between these facts was unknown. Thus before Dalton's time, although a chemist might predicate the kind of action which occurs when two chemical substances are brought together, no one could calculate with precision how much of each ingredient is required to build up the

new body. After Dalton's work such a calculation became easy and certain. Hence it is that Dalton may truly be said to be the founder of modern Chemistry. As with the indestructibility of matter, so with the indestructibility of energy. What Dalton did for the first great principle, Joule accomplished for the second; and he is therefore the founder of modern Physics. And thus the great twin brethren of Manchester did work for the world the like of which hath not been seen, and the importance of which cannot be reckoned.

It is sufficiently remarkable that England should have produced two such men; still stranger might it seem that they arose in the midst of a population given up to industrial pursuits, where the noise of the loom and the spinning-jenny was heard in the land, and where most men's thoughts were engrossed in what shallow minds often look upon as common trade avocations. But quiet corners were in those days, and are even now, to be found in Manchester; and it may well be argued whether the distant roll of the loaded waggon, the hum of the mill, or even the screech of the locomotive, with all the stir and energy at work within the busy hive of a great

industrial centre which those sounds imply, are not, after all, conducive to work of another and a more abstruse character. Whether, in fact, Dalton and Joule would have accomplished their life-work more fully had they been born to the intellectual purple of the ancient universities, and had to spend their time amidst the, to many minds, somewhat enervating influences of college life, instead of in the more robust and stimulating air of sturdy northern independence and intelligent northern activity.

If, as it is said, the world is ruled by ideas, nothing can surely be more appropriate than the position, in the centre of the municipal life of the northern metropolis, accorded to the statues of these two Manchester men. For upon the work of Dalton and Joule depends success or failure, according as the principles laid down by them are respected or neglected, of all the thousand and one industrial undertakings of that, as of every other community, which follow either the chemical change of one form of matter into another, or the conversion of heat, electricity, or other forms of energy into mechanical work or *vice versâ*.

Just as the different branches of science overlap,

and as there is no strictly dividing-line between them, one following on and depending on the other, so the work of one of these Manchester men follows naturally on that of the other. Joule was the pupil and the scientific son of Dalton; he inherited the scientific spirit, and carried out the methods of investigation of his master with added refinement and knowledge.

It is with the life and work of the elder of these two giants of science that we have in these pages to do. In telling the life-tale of scientific men, the biographer has often little to do beyond chronicling their researches and noting the influence which their work exerted on the progress of natural knowledge. Most of such men live uneventful lives; their personal history is not unfrequently of but slight interest to the general reader; sometimes it is even commonplace; their work has lain in the laboratory or the observatory, where the even tenor of their days is only broken by the discovery of a new law, of a new element, or of a new planet.

In Dalton's case this rule does not apply. For although so devoted to his science that he used to say he had no time to get married, and although the

greater part of his life was uneventful, spent as it was in working and teaching in a more or less humble way in a provincial town, yet his character presents so many varied aspects, and exhibits such originality, that, apart altogether from his scientific labours, and independently of his position as one of the world's great chemists, the life of the individual man is a study full of interest.

Several memoirs of John Dalton have been published. The first by his friend and pupil, the late Dr. William Charles Henry, F.R.S., printed for the Cavendish Society. A second by the late Dr. Angus Smith, F.R.S., published by the Philosophical Society of Manchester, of which Society Dalton was so long the distinguished President. And a third by my late talented friend Dr. Lonsdale, of Carlisle—being one of his series of Lives of Cumberland Worthies; for Dalton, as we shall see, was born in that county, although he passed most of his life in Manchester. In this last volume the story is so clearly told, and the pages so often lightened by the true Cumbrian humour of the writer, that I cannot expect to do much more than follow where he has led, and place before, I hope, a somewhat larger

audience than he appealed to, and in a more condensed form, the chief points of interest in John Dalton's life, both as a man and as a chemist.

The following is a complete list of memoirs and of notices of Dalton's life:—

W. C. Henry, "Memoirs of Life and Scientific Researches of J. Dalton" (1854).
R. A. Smith, "Memoir of J. Dalton" (1856).
H. Lonsdale, "Worthies of Cumberland: Dalton" (1874).
J. Harland, "Manchester Collectanea" (Vol. II. p. 232).
C. Wheeler, "Manchester" (1836).
F. Espinasse, "Lancashire Worthies." Second Series.
"Annual Monitor" of Society of Friends (1845, p. 40).

JOHN DALTON

AND

THE RISE OF MODERN CHEMISTRY.

CHAPTER I.

EARLY LIFE.

PASSING in the train from Carlisle to Cockermouth, after a journey of about thirty miles, the station of Brigham is reached, and thence a short walk leads to the village of Eaglesfield, where John Dalton was born about the 6th of September, 1766. There stands the first Meeting-house established in Cumberland by the Society of Friends, and there under the grassy turf lie the forbears of the Dalton family, for on both sides Dalton's parents were Quakers. The house in which he was born was a thatched cottage; now its exterior has been modernised; but inside the dwelling is still much in the condition in which it stood one hundred and thirty years ago, when occupied by Joseph Dalton, the hand-loom weaver, and his "gudewife" Deborah, the parents of the chemist.

Lately, it is satisfactory to learn, a tablet recording this cottage as Dalton's birthplace has been placed by some appreciative friends on its walls. Like most of the Cumberland cottages, a porch stood in front of the door, with slabs of slate on which the pans and other household vessels are placed to air. On opening the door a small square kitchen or "house-part" is seen, where the family lived, and where Joseph Dalton had his loom. Further on is a small sleeping-room, fifteen feet long by six feet high and wide. Here may be seen the recess in which was placed the chaff bed where the couple lay, and here their son John was born. The craft of the father was one common enough in the north of England a century ago. All the country people wore home-made cloth and home-spun linen. The "grey coats of Cumberland" was a well-known appellation for sturdy Liberal opinion, and many a political battle was in those days fought in the "old grey stuff." For ordinary wear the wool was subjected to little dressing, but spun on the "large wheels" found in every cottage, and then woven into stuff on the loom, with which Joseph Dalton earned a somewhat precarious pittance. In spite of his modest earnings — for he seems, as Lonsdale says, to have been a "feckless" kind of man — he pricked up courage to go as far as Caldbeck to court Deborah Greenup, who came of an old "Statesman" Quaker stock, and whom he married at Cockermouth Meeting-house on June 10th, 1755.

Deborah was of an active habit, and from her, rather than from his father, it doubtless was, as is generally said to be the case, that Dalton inherited much of his peculiar power. Three children of the marriage lived to a good old age, three others died early. The three who lived were Jonathan, Mary, and John. Jonathan was the eldest son; but where in the family John came no one knows, for singularly enough, no register of his birth has been found. The Quakers do not practise any baptismal rites, although the births of their children are, as a rule, duly registered. There was, of course, no entry of the birth in the Parish Church books, and so the date of John's entrance into the world remained undiscovered, and was unknown to himself, until after he had become famous, when some inquiries at his birthplace elicited the fact that he probably first saw light on the 6th September, 1766.

A century and a half ago the Cumbrian Quaker schoolmasters were far superior in mental capability and in scholastic discipline to the ordinary run of north-country teachers. To such a master Dalton was fortunately sent, and to the early training which he received from Mr. Fletcher he himself ascribes much of his subsequent success in life. Not a brilliant or quick boy, Dalton showed in these first years germs of that steadfastness of purpose and power of abstract thought which made him great. In a letter written in 1832, Dalton himself describes this part of his early life as follows:—

"The writer of this was born at Eaglesfield, near Cockermouth, Cumberland. Attended the village schools there, and in the neighbourhood, till eleven years of age, at which period he had gone through a course of mensuration, surveying, navigation, etc."

The Quaker schoolmaster, Fletcher, was evidently a superior kind of man, and not one of the old sort who hammered the Latin grammar into the boys' heads by a process of birching elsewhere. Who knows whether the boy of nine, if he had received such a treatment instead of being put on to mathematics, the foundation of all natural knowledge, would ever have left his native uplands.

But this was not to be, and young Dalton not only attracted the attention of his schoolmaster, but that of another remarkable Eaglesfield man, who first became his patron and then his fast friend for life. And nothing strikes one as more remarkable in this remarkable life than that in this remote Cumberland village should be found, in the middle of last century, a Quaker gentleman of means, of scientific and literary ability far above the average of men living in more favoured centres. Mr. Elihu Robinson was a capable meteorologist, probably the first in Cumberland; he was also a skilled instrument-maker, and corresponded with men like Franklin and Clarkson. Indeed, the existence of such highly cultured, liberal-minded Quakers was no uncommon thing in West Cumberland in those early days. Earnest in well-doing, these

men stood in the forefront of every liberal movement, and exerted a beneficial influence not only in their own district, but over a much wider area. The friendship and intercourse enjoyed by the poor weaver's son with Elihu Robinson were conditions admirably suited to advance and stimulate Dalton's powers; for he assisted the boy in his studies, and taught him, along with a young man twice his age but with not half his power. Mr. Robinson would often set the boys questions in mathematics, and after they had puzzled over the problem for some time, Dalton's companion would propose to ask for the solution, whereat Dalton, showing the reliance on his own powers which was one great characteristic of his life, proved his determination to persevere by saying in his Cumbrian dialect, "Yan med deu't" — a text upon which many a good sermon might be preached. One day, it seems, a dispute as to the best solution of a problem arose, and a bet was proposed; but as the stern Quaker preceptor objected to such a proceeding, it was agreed, though one hardly appreciates the moral difference, that the loser should pay for his companion's candles during the winter — probably they were "farthing dips" — and Dalton won. Another time Mr. Robinson set the boys some stiffish problem, and after a while asked, "Now, John, hast thou done that?" "No," said John, "but yan med deu't." Again, after the lapse of an hour, the same question was asked. "No," he said, "I can't deu't to-neet, but mebby i' th' morn I

will"; and, right enough, sleep did the work for him, and the correct answer was forthcoming in the morning.

The progress which the boy made under these stimulating influences must have been great, for, in his own words, he "began about twelve to teach the village school, and continued it two years." How he began to teach is thus recorded: — On the outside of his father's cottage, or on the front door, the boy nailed a large sheet of white paper, on which was inscribed in a bold hand the fact that John Dalton had opened a school for both sexes on reasonable terms, and an additional piece of information indicating that "paper, pens, and ink" — articles then not frequently met with in Eaglesfield — were purchasable within. To begin with, this original school, with a "Principal" twelve years of age, seems to have been carried on in an old barn, then in his father's cottage — much, one would think, to the annoyance of both the weaver and the pupils — and later in the Quakers' Meeting-house; for these admirable people, unlike some far more powerful sects, see no objection to their "House of God" being used for purposes which have for their aim to make the children of men fitter occupants of the kingdom of heaven.

Some strange scenes were witnessed in this school. The boy's pupils were of all ages, from infants to big boys and girls of sixteen or seventeen. The infants sat on the master's knee to learn their "a b — ab."

The big boys were often not only crassly ignorant, but some were rough and bad. John Dalton, in the exercise of his authority, desired to chastise these offenders; but this was sooner said than done. The big boys rebelled against the small "Principal," and —*horribile dictu!*—offered to fight him! How the matter ended is a subject upon which history is silent, but we may well imagine that the pluck and Quaker firmness of the young master was equal to coping with the bluster of the ignorant and ill-bred bully.

The emoluments coming to the "head" of the school were not large—probably five shillings a week was the maximum—so that we need not wonder at the lad wishing to increase his earnings, or at finding that, after schoolmastering for a couple of years, he turned his hand to the plough and worked as a labourer on a small patch of land which his father farmed. Possibly the fact that his uncle, who was a well-to-do farmer in the neighbourhood, and who had no sons, might make his favourite nephew his heir and successor may have induced Dalton to take to husbandry—an occupation, in those days, quite as honourable and more lucrative than that of a village schoolmaster. He might have remained a "statesman," selling his butter and cows in the neighbouring market; but the forces of nature—whatever that means—were against it, and the boy became a man whose name goes down to posterity as one of the great ones of the earth.

Physical work on the farm during the day gave the boy a zest for intellectual occupations in the evening, when he and his companions sat over the fire breaking their heads about Mr. Robinson's puzzles, and when John worked steadily at the problem until the riddle was solved. Thus, both physically and mentally, his stature grew and developed into a man firm of tissue and of will.

When he was fifteen, in 1781, he left his native village for ever. His elder brother had already settled in the county town of Kendal as assistant or usher to a cousin, George Bewley, also a Quaker, who had established a school for "Friends" of both sexes in that town. Brother Jonathan, possibly foreseeing Bewley's speedy retirement, proposed to John to join him with a view to carrying on the school together. So, after the manner of their sect, "Friends" were consulted by the parents on the desirability of this step being taken by their son, and, the oracle having pronounced a favourable judgment, the boy set off for Kendal.

His outfit was not a luxurious one. It is said that he first saw an umbrella in a shop in Cockermouth and bought one, thinking, as he afterwards expressed it, that he was becoming a gentleman; and, with this in one hand and with a bundle of underclothing in the other, John Dalton started for his walk of forty-four miles. His way lay through some of the most beautiful parts of that most lovely of districts, the

English Lakes. The surroundings of his native village were plain and bleak — true, from the cottage door he could discern the peaks of Skiddaw and Saddleback standing up above the moor; but that was all. Soon, however, he found himself under Skiddaw's shade, with smooth Bassenthwaite at his feet, and Derwentwater, with its bosky islets and fringe of lovely Borrowdale mountains, as a background.

The glorious scenery of the lake district must have made a deep impression upon the youth, for ever afterwards, and up almost to his latest day, this was the happy hunting-ground of the man of science. Here, as we shall see in due course, he came almost every summer for many years, and here he made some of his most important meteorological observations, often climbing the dark brow of mighty Helvellyn with his home-made barometers and thermometers, and taking with him bottles to fill with the air from that high altitude, upon the analysis of which he founded more than one of his scientific conclusions. Passing first by Thirlmere, from which in these latter days Manchester draws its supply of the purest of waters, and grazing the edge of Rydal and Grasmere — since that time the loved retreat of the Lake Poets — he came, past Waterhead, upon Windermere, and leaving this Queen of the Lakes and passing over the fells which divide the watershed, Dalton reached the market town of Kendal. Here he found a community of some 5,000 souls driving a flourishing

trade in homespuns and "Kendal-green" cloth, the packs being taken to Liverpool by hundreds of horses.

In those days a stage-coach arrived at Kendal twice a week from London, but the "flying machine" did not carry mails till 1786. At that time, too, though churches and schoolhouses were being built, the amusements of the people were still of the coarser kind, for public bull-baiting was in full force until 1791, when it was prohibited by order of the Corporation, probably on the instigation of influential members of the Society of Friends, who took then, as they do now, a leading part in all that regards the education and culture of the townsfolk.

John served his brother and his friend for four years, and in 1785 Bewley retired, and the brothers Dalton carried on the school, "where youth" of both sexes "will be carefully instructed in English, Latin, Greek, and French; also writing, arithmetic, merchants' accompts, and the mathematics." A photographic copy of the card containing this announcement, from the original in the possession of Mr. George Woolley, of Manchester, has kindly been made for this volume by Mr. Brothers, the well-known photographer.

JONATHAN and JOHN DALTON,

Refpectfully inform their Friends, and the Public in general, that they intend to continue the SCHOOL lately taught by

GEORGE BEWLEY,

Where Youth will be carefully inftructed in

Englifh, Latin, Greek, and French;

ALSO

Writing, Arithmetic Merchants Accompts,

And the MATHEMATICS.

The School to be opened on the 28th of March, 1785

N. B. Youth boarded in the Mafter's Houfe on reafonable Terms.

Kendal. Printed by W. Pennington.

DALTON'S CARD.

A second circular sent out in the following year shows that the brothers had enlarged their views on education, and the syllabus forcibly reminds us of that of a technical school of the present day. It is as follows: —

"Kendal, July 5th, 1786.

"Jonathan and John Dalton take this method of returning their acknowledgments to their friends and the public for the encouragement they have received since their opening school; and, from their care and assiduity in the management of it, manifested in the improvement of the youth under their care, are induced to hope for a continua-

tion of their favours. They continue to teach, on reasonable terms, English, Latin, Greek, and French;

also

Writing,	Mensuration,	Projections,
Arithmetic,	Surveying,	Dialling,
Merchants' Accounts,	Gauging,	Optics,
Geometry,	Algebra,	Mechanics,
Trigonometry,	Fluxions,	Pneumatics,
Navigation,	Conic Sections,	Hydrostatics,
Geography,	Astronomy,	Hydraulics, &c.

"N.B.—Persons desirous of being instructed in the use of the globes, &c., will be waited upon any time out of school hours. The public may also be informed that they could conveniently teach a considerable number more than at present. Those who send their children may depend upon their being carefully instructed."

The Daltons also offered to take in boarders, and their sister Mary came from Eaglesfield to keep house for them, and the parents, now old people, often walked "over fell and slack" a distance of forty-five miles to see their sons. The fee charged was ten and sixpence per quarter; but in 1811 this was raised by Jonathan, who was the head master, with the expressed hope that "a small advance (the increased price of the necessaries of life considered) would not be thought unreasonable." The capital at the disposal of the brothers was not large; indeed, it was so small that on one occasion they had to borrow two or three pounds from Mr. Bewley, and even from their poor parents, to enable them to carry on their enterprise. They took care of

their money, balanced their books every month, and put down every penny they spent. Mary lent them thirteen shillings and sixpence, and got paid in instalments: "Mary, in part, £0 0*s.* 6*d.*" The first year's income was the largest, and only amounted to £100, so the brothers sought to increase their means by collecting rents, and making wills, in order that they might be able to keep their three selves and to preserve a decent appearance.

Mrs. Cookson, who was a pupil in the school in 1785, writes to Dr. John Davy (the brother of Sir Humphry, and the discoverer of chlorcarbonyl, or phosgene gas, as he termed it), who lived at the Lakes, the following reminiscences of the school, which contained some sixty boys and girls: "The school was not generally popular, owing to the uncouth manners of the young masters, who did not seem to have had much intercourse with society; but John's natural disposition being gentler, he was more passable. I believe the last time of my going to Mr. Dalton was about the year 1789. He was then become rather more communicative in his manner, but still a man of very few words."

John was considered to be very studious. He often made calculations on slips of paper which the girls found torn up and thrown down near his desk. The brothers lived a very secluded life, and Dr. Davy opines, probably with truth, that their manners must have been more than usually hard and ungainly.

A letter from another pupil at the Kendal School — a Mr. Isaac Braithwaite, a well-known north-country name — throws further light on Dalton's character and habits. He "recollects the boys being all glad to be taught by John Dalton, because he had a gentler disposition; and, besides, his mind was so occupied with mathematics that their faults escaped his notice."

John Dalton kept school at Kendal for twelve years. During this time he was unceasingly engaged in self-improvement, and some of his methods of doing so are so different from those adopted at the present day that they deserve a passing notice. At that period one of the few semi-educational periodical publications was the *Ladies' and Gentlemen's Diary and Woman's Almanac*.* He had seen some of these numbers when living at Eaglesfield, and had, when thirteen years of age, copied out the whole of the almanac for the year 1779. At Kendal he carried out his love for grappling with the problems which these publications contained, many of which involved the knowledge of most of the branches of mathematics then cultivated by English geometers. Prizes were awarded by the publishers of the magazine to those who solved the greatest number of questions. In 1787 fifteen such problems were proposed, and John Dalton successfully answered thirteen

* Edited, it is believed, by Dr. C. Hutton, of the Royal Military Academy.

and obtained the prize. His replies to the questions of the following year were equally satisfactory, and they showed that he possessed not only high mathematical powers, but also that he was able to answer general questions on natural science, and had a knowledge of chemistry, and was acquainted with the writings of contemporary French chemists. In 1789 he was awarded the prize of six *Ladies' Diaries*, and in the *Gentlemen's Diary* for that year we find the announcement that he had gained the enviable position of first place amongst the correspondents to that journal as having solved a difficult question on hydrostatic equilibrium.

But Dalton did not always confine his attention to answering mathematical questions; sometimes he ventured on ethical ground, and some of his replies, quoted by the late Dr. George Wilson in the *Quarterly Review*, are amusing enough to bear repetition.

I.—Query by William Gradidge, of Canterbury.

"Whether, to a generous mind, is the conferring or receiving an obligation the greater pleasure?"—Diary for 1791, p. 32.

Answered by Mr. John Dalton, of Kendal.

"The pleasure arising from conferring an obligation, especially if it be effected without much inconvenience,* is

* Whether this saving clause is intended to be satirical mnst be left to the judgment of the reader.

pure, and must be a grateful sensation to a generous mind; but that arising from receiving an obligation is often mixed with the unpleasing reflection of inability to remunerate the benefactor. It is pretty clear, therefore, that the pleasure of conferring an obligation must exceed that of receiving one." — Diary for 1792, p. 24.

II.— Query by Mira.

"Is it possible for a person of sensibility and virtue, who has once felt the passion of love in the fullest extent that the human heart is capable of receiving it (being by death, or some other circumstance, for ever deprived of the object of its wishes), ever to feel an equal passion for any other object?" — Diary for 1791, p. 32.

Answered by Mr. John Dalton, of Kendal.

"It will be generally allowed that in sustaining the disappointments incident to life true fortitude would guard us from the extremes of insuperable melancholy and stoic insensibility, both being incompatible with your own happiness and the good of mankind. If, therefore, the passion of love have not acquired too great an ascendency over the reason, we may, I think, conclude that true magnanimity may support the stroke without eventually feeling the mental powers and affections enervated and destroyed by it, and consequently that the query may be answered in the affirmative. However, if this passion be too strong, when compared with the other faculties of the mind, it may be feared that the shock will enfeeble it so as to render the exercise of its functions in future much more limited than before." — Diary for 1792, p. 24.

In Kendal, as at Eaglesfield, Dalton was singularly fortunate in securing the friendship of persons of similar tastes and character. In this instance it was

to a blind man that young Dalton was indebted for a close and valuable friendship. The importance which he attached to the assistance in his studies and the aid in forming his habits of investigation which he obtained from blind Gough, Dalton acknowledges in the preface to his Meteorological Observations and Essays published in 1793. "To one person more particularly I am peculiarly indebted, not only in this respect, but in many others; indeed, if there be anything new and of importance to science contained in this work, it is owing, in great part, to my having had the advantage of his instruction and example in philosophical investigation." Dalton explains more fully the character of his friend in the following letter written by him during Mr. Gough's lifetime to Mr. Peter Crosthwaite, of Keswick:—

"KENDAL, April 12th, 1788.

"John Gough is the son of a wealthy tradesman in this town. Unfortunately he lost his sight by the small-pox when about two years old, since which he has been quite blind, and may now be about thirty. He is, perhaps, one of the most astonishing instances that ever appeared of what genius, united with perseverance and every other subsidiary aid, can accomplish when deprived of what we usually reckon the most valuable sense. He is a perfect master of the Latin, Greek, and French tongues: the two former of which I knew nothing of six years ago, when I first came here from my native place near Cockermouth, but, under his tuition, have since acquired a good knowledge of them. He understands well all the different branches of mathematics, and it is wonderful what difficult and abstruse problems

he will solve in his own head. There is no branch of natural philosophy but what he is well acquainted with; he knows by the touch, taste, and smell almost every plant within twenty miles of this place; he can reason with astonishing perspicuity on the construction of the eye, the nature of light and colours, and of optic glasses; he is a good proficient in astronomy, chemistry, medicine, etc., etc. His father, being very able, has furnished him with every necessary help, and would have sent him to the university had he been inclined. He has the advantage of all the books he has a mind for, which others read to him; he employs one of his brothers or sisters to write for him, or else myself, especially in mathematical attempts; he has never studied the art of writing much, or I doubt not he would succeed. He and I have been for a long time very intimate. As our pursuits are common — viz., mathematical and philosophical — we find it very agreeable frequently to communicate our sentiments to each other and to converse on those topics."

After Gough's death Dalton, in the preface to a second edition of the above-named work, speaks even more freely of his sense of his obligations to his blind friend: —

"Mr. Gough might justly be deemed a prodigy in scientific attainments, considering the disadvantages under which he laboured. Deprived of sight in infancy (one or two years old) by the small-pox, he was destined to live to an advanced age under this which is commonly reputed one of the greatest misfortunes that can fall to the lot of man. In his case, however, it may fairly be questioned whether he would have had more enjoyment in himself and have been of more use to society in the capacity of a manufacturer, his probable destination, than in that which was allotted to him. By the mathematical education; and, naturally possessing great

powers of mind, he bent them chiefly to the study of the physical and mechanical sciences. There are few branches of science in which he did not either excel or of which he had not a competent knowledge: astronomy, optics, pneumatics, chemistry, natural history in general, and botany in particular, may be mentioned. For about eight years during my residence in Kendal we were intimately acquainted. Mr. Gough was as much gratified with imparting his stores of science as I was in receiving them. My use to him was chiefly in reading, writing, and making calculations and diagrams, and in participating with him in the pleasure resulting from successful investigations; but, as Mr. Gough was above receiving any pecuniary recompense, the balance of advantage was greatly in my favour, and I am glad of having this opportunity of acknowledging it. It was he who first set the example of keeping a meteorological journal at Kendal."

In subsequent years Dalton made a point of submitting to Gough his own work; and Gough, especially in the case of Dalton's celebrated investigations on the constitution of mixed gases — about which I shall have more to say hereafter — endeavoured in no very friendly tone to controvert Dalton's resnlts, insinuating that Dalton had made his theory to fit in with his inexact observations; for "the human mind is naturally partial to its own conceptions, and frequently condescends to practise a little self-delusion when obliged by the force of facts and arguments to abandon a favourite notion," and so forth. Dalton, mindful of his former indebtedness to Gough, replies to these somewhat intemperate remarks by simply stating that "in discussions relative to experimental

philosophy we expect facts opposed to facts, and arguments to arguments; whereas in the present instance Mr. Gough has done little more than insinuate that my instruments are inaccurate, and that the results of my experiments are unfaithfully represented, without in any one instance bringing either of these charges home to me."

The following letter, written by Dalton from Kendal to his former companion, Alderson, of Eaglesfield, throws a further light on his character, and is, therefore, worthy of being quoted:—

"Kendal, 8 mo., 4th, 1788.

"Respected Friend,—Happening a while ago to be in company where the topic of conversation was the derivation of surnames, a subject quite new to me, and being, as thou may remember, inquisitive into things seemingly involved in mystery, and which require some sagacity to unravel, I could not help afterwards reflecting a little upon it. The substance of my reflections, and the information I could get, being put to paper will run nearly as follows. There is very little utility arising from the subject, but a small matter of curiosity, which I thought might not be altogether unacceptable.

"Anciently in this kingdom it seems to have been customary to have only one name — that is, what is now called the Christian name; and, that not being sufficient for distinction, others were added to it such as were most fit to answer that end — such as whose son a person was, what trade he was, where he came from, etc. — which, however, were subject to change, according to the caprice of the neighbourhood or fancy of the person, till the Legislature found it necessary that they should be fixed, to prevent the evils that might otherwise arise."

The list of names is too long to be here reproduced; but it may be well to note that he gives the derivation of his own name as being from Dalton, a village in Lancashire, meaning "Daletown," and also gives that of his correspondent, in concluding the letter, as follows: —

"However, as I have explained my own name, I must do the same with thine. Alderson means undoubtedly 'olderson,' *old* being pronounced *ald* in this county, where possibly the name originated; but it is not easily made appear how such a name arose. — Please to accept the best respects of thy friend.

"JOHN DALTON."

Following the lead of Robinson in Eaglesfield, and of Gough in Kendal, Dalton's first attempts at scientific investigation were meteorological. The archives of the Literary and Philosophical Society of Manchester contain two volumes of Dalton's "Meteorological Journal," being the observations made in Kendal from 1787–93 and those made in Manchester 1793–1803, and these daily observations were continued without a break from that time until the evening before his death in 1844. It is interesting to note that the first entry in the journal — that for March 24th, 1787 — was a description of an auroral display which he witnessed — a phenomenon which, as we shall see, afterwards often attracted his special attention.

In these first beginnings, as in his later scientific work, Dalton used home-made instruments, often crude and inaccurate according to modern notions. "The preceding and following observations on the temperature of the weather were made on instruments of my own construction. The barometer is graduated into sixteenths of an inch. The thermometer is mercurial, with Fahrenheit's scale, exposed to the open air, but free from the sun. The hygroscope is about six yards of whipcord suspended from a nail, with a small weight to stretch it, its scale, tenths of inches, beginning from a certain point — the less the number, the shorter the string and the greater the moisture." In those days reliable barometers were not as easily obtained as now; and Dalton not only made such instruments for his own use, but sold them to others. Thus he furnished Mr. Crosthwaite, the founder of the Keswick Museum, which still exists, with a barometer and a thermometer, for which he charged him 18s. and 5s. In his letter accompanying the instruments he describes minutely how he made the barometer, and it is clear that he not only did not boil the mercury nor heat the glass tube, but did not even warm both. So that no precautions were taken to remove the adhering moisture and air. This omission Dalton seems to have become aware of, for he writes afterwards as follows: —

"I intend to renew mine as soon as convenient. If thou do the same, be careful in undoing it, and attend to the cautions I give: — Be sure to rub the inside of the tube well with warm dry cotton or wool, and have the mercury, when poured in, at least milk-warm — for moisture is above all things else to be avoided, as it depresses the mercury far more than a particle of air does. Mine is, as I have said, at least $\frac{1}{16}$th of an inch too low, and yet it is clear of air, and, to all appearances, dry; but I doubt not but attending to these precautions, *which I knew nothing of when it was filled*, will raise it up to its proper height."

Again in January, 1793, he observes: —

"I consider both our barometers as inaccurate with respect to the distance of the *basins and scales;* but this is of little importance provided they be true in other respects. This only serves to show the relative heights of the places to the sea, which we can come at better by other means."

How he made his thermometers is described as follows in a letter addressed to Elihu Robinson, his early friend and instructor: —

"KENDAL, 8 mo., 23rd, 1788.

"DEAR COUSIN, — Herewith thou wilt receive, I hope safely, two thermometers, with somewhat longer scales than the former. Please to take thy choice of the three, to let John Fletcher have next choice, and to reserve the other till my brother comes.

"You will probably chuse by the length of the scales; but those with the least bulbs will soonest come to the temperature of the surrounding medium. However, the largest, I apprehend, will rise or fall to within a degree of the proper place in half an hour in the air. Thou may try whether that thou hast already is with these two or not by dipping the bulbs into a basin of hot water for five minutes.

"Possibly the manner of making them may not be unentertaining. A small receptacle being fixed on the end of the tube, a quantity of mercury is poured into it, part of which runs down the tube so as to fill half the bulb. Then a candle is applied to the bulb, which, rarefying the air contained in it, raises the mercury in the tube quickly to the top, and then it escapes in the bubbles through the mercury in the receptacle. This done, it is cooled again, when, the internal air contracting, another portion of mercury falls down into the bulb; and this operation is repeated till all the air is expelled. Then the mercury is heated above boiling water, and the end of the tube melted and closed at the same time, when, the mercury subsiding, there is left a vacuum. This is done chiefly to keep the moisture, dust, etc., out of the tube. The whole is then put into boiling water, when the barometer stands at thirty inches, and the boiling point thereby determined. Afterwards (if circumstances admit) the freezing point is found by putting it into a mixture of water and pounded ice, or water and snow, which, when melting before the fire, keep at an invariable point (32 deg.) till the whole is melted. If this cannot be done as in summer, it may be set by another thermometer, and the scale adapted accordingly. N.B.—As the freezing points of these two were not found on account of the season, it will not be amiss to try whether they are accurate when a convenient season comes.

"The principles on which they act need little explication, as mercury, like most other bodies, is subject to be contracted by cold and expanded by heat; and as the capacity of the bulb remains always filled, the total variation of the mercury in bulk, it is evident, will be manifested in the tube.*

"The range of the thermometer is little in these parts compared with the more northern. At Petersburgh the summer heat is equal to ours, but in winter severe cold predominates. The thermometer is frequently found 40 or 60 below nothing; and in Siberia it has been observed

* In this communication Dalton takes no account of the expansion of the glass bulb.

even 100 or 120 below nothing. On the contrary, in the burning sands of Africa it reaches 120 or 140 above nothing. Is not the internal principle of heat in man and other animals a wonderful phenomenon that can sustain these two extremes without sensible variation? Remark. — Réamur's scale (used by the French and others) counts from 0 at the freezing-point to 80 at the boiling-point. Consequently, $2\frac{1}{4}$ degrees Fahrenheit are equal to 1 of Réamur.

"ABSTRACT OF MY JOURNAL FOR THE PRESENT YEAR.

Thermometer without.				Rain. Inches and Decimals.	Wet Days.	Auroræ Boreales.
	Mean.	Highest.	Lowest.			
1 mo.	39·0	47	20	5·6160	20	6
2 mo.	38·3	47	28	3·3064	23	2
3 mo.	36·8	50	18	2·8183	16	4
4 mo.	46·3	69	32	2·9047	16	11
5 mo.	53·0	80	38	1·1872	10	7
6 mo.	57·3	80	45	2·3137	7	2
7 mo.	56·8	68	47	7·0323	28	1

"THUNDERSTORMS.

"5 mo., 19. 2 p.m. distant W.

" ,, 26. 7 p.m. frequent loud peals, very near.

"7 mo., 3. 6 p.m. frequent peals, some very near.

"8 mo., 16. $7\frac{1}{2}$ p.m. distant about eight miles S.E., but loud and tremendous. About twenty or thirty flashes were observed in as many minutes, and the reports of each heard, though the cloud was but just visible above the horizon; the zenith clear. — My love to Cousin Ruth, self, and family. JOHN DALTON."

These barometers were not only used for ascertaining the daily or monthly range, but also for measuring the heights of the Lake Mountains, which Dalton first carried out on the hills above Kendal in 1787.

The Lake district is not an unfavourable one for measurements of rainfall; for there the statement that "the rain it raineth every day" is truer than in any other part of the island, nor is its quantity wanting; at Seascale, in Borrowdale, the fall is almost tropical, reaching an average of 160 inches, the mean rainfall for the kingdom being 37·3.

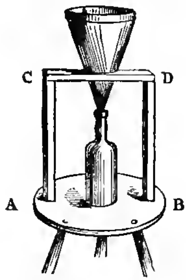
Eaglesfield, in days long before girls' high schools or sweet girl graduates were heard of, could evidently boast of young women who took an interest in and were conversant with natural phenomena, for we find Dalton writing to Miss Hudson, an old pupil, a letter in which decimal fractions — a snare and a stumbling-block even to some "great" men of the present day — were made use of. This is worth reading, as showing his method of ascertaining the depth in inches of rain falling at any station from the weight of water collected in the gauge, and as also giving an idea of what a village girl in a remote part of the country was expected to understand some more than a century ago. Could the same thing be looked for in a similar district now, in spite of Education Codes and Technical Instruction Acts?

"KENDAL, 8 mo., 4th, 1788.

"RESPECTED FRIEND, — The study of nature having been with me a predominant inclination, it is not likely that I should be ready to prompt others to the same. I have been tempted to think that thou would take a pleasure in remarking the quantity of rain that falls with you each day, if thou knew with what facility the same

is effected. I have observed here that people who are entirely ignorant of the matter suppose it a work of great labour and difficulty, and which can only be done by those they call great scholars. This, however, is a great mistake. A very little knowledge of mensuration is sufficient for the theory of it, and nothing but plain addition is wanted in the practice.

"The annexed scheme will represent the most simple apparatus: — AB is a three-foot stool, to be fixed in a garden bed, etc.; AC and BD two posts fixed in the same about 11 or 12 inches, and support the arm CD, which is $1\frac{1}{2}$ inch



broad and 1 deep. The pipe of the funnel exactly fits the hole in CD, keeping the funnel firm and level. The funnel may be 6, 7, or more inches over; and if it have an upright rim of an inch it is better, but will do without it. Also, it should be painted, to save it from the weather. A common glass bottle will hold all the water that falls at any time in twenty-four hours, if the funnel be one only six or seven inches diameter — except, perhaps, two or three days in the year. A pair of scales, with a few small weights, are requisite.

"Now, to determine the depth of water that falls on any level surface from the above, we have the following tables made for funnels of six and seven inches, wherein are set down the depths, corresponding to the several weights, in decimal fractions. And any person who has learned mensuration will be able to adapt a table to any funnel by knowing that $62\frac{1}{2}$ lbs. avoirdupois equal 1 cubic foot of water.

"Suppose there is caught with a funnel of six inches diameter 1 lb. 3 ozs. $5\frac{1}{4}$ drs. of water, required the depth:—

1 lb.	=	·9778
2 ozs.	=	·1222
1	=	·0611
4 drs.	=	·0153
1	=	·0038
$\frac{1}{4}$	=	·0010
		1·1812

That is, the depth that would on a level surface be 1 inch, 1 tenth, 8 hundreds, 1 thousand, and 2 ten thousand parts of an inch.

Weights.	Diameters of Funnels.		Weights.	Diameters of Funnels.	
lb. av.	6 inches.	7 inches.	Av.	6 inches.	7 inches.
1	·9778	·7184	4 drs.	·0153	·0112
8 ozs.	·4889	·3592	2	·0076	·0056
4	·2445	·1796	1	·0038	·0028
2	·1222	·0898	$\frac{1}{2}$	·0019	·0014
8 drs.	·0306	·0225	$\frac{1}{4}$	·0010	·0007
			$\frac{1}{8}$	·0005	·0004

"Suppose with a funnel of seven inches there is caught 1 oz. $7\frac{1}{2}$ drs.

1 oz.	=	·0449
4 drs.	=	·0112
2	=	·0056
1	=	·0028
$\frac{1}{2}$	=	·0014
		·0659

That is, 6 hundredth, 5 ten-hundredth or thousandth, 9 ten-thousandth part of an inch.

"N.B.—The water is supposed to be taken at stated hours, as 6, or 8, or 10 at night.

"By this time I apprehend the difficulty generally supposed to attend this matter is removed. I should be glad if thou, or any other in your neighbourhood, on whose accuracy one might rely, would find it agreeable and convenient to notice this matter; but, however, I do not mean to request it, but only to show the easiness with which it's done. Ignorance, no doubt, will look upon this as a trifling and childish amusement, but few of this nature are such in a philosophical sense. If to be able to

predict the state of the weather with tolerable precision, by which great advantages might accrue to the husbandman, to the mariner, and to mankind in general, be at all an object worthy of pursuit, that person who has in any manner contributed to attain it cannot be said to have lived or to have laboured in vain. — I am, respectfully, thy friend,

"JOHN DALTON.

"To Sarah Hudson, Eaglesfield."

In 1787 Dalton tried his hand at public lecturing, and a course was announced on October 26th of that year in the following terms: —

"Oct. 26th, 1787.

"Twelve lectures on Natural Philosophy to be read at the school (if a sufficient number of subscribers are procured) by John Dalton. To begin on Tuesday evening, the 13th November next, at six o'cl., and to continue every Tuesday and Thursday at the same hour till compleated.

"Subscribers to the whole, half-a-guinea; or one shilling for single nights.

"N.B. — Subscribers to the whole course will have the liberty of requiring further explanation of subjects that may not be sufficiently discussed or clearly perceived when under immediate consideration; also of proposing doubts, objections, etc., all which will be illustrated and obviated at suitable times to be mentioned at the commencement.

"A SYLLABUS OF THE LECTURES.

"*First and Second — Mechanics.*

"Introduction. Rules of philosophising on matter and its properties, with the different opinions of the most famous philosophers on this head.

"The laws of motion. Mechanic powers. Vibration of pendulums.

"*Third, Fourth, and Fifth — Optics.*

"Preliminary discourse. Of the nature and properties of light. Of simple vision. Doctrine of colours. Of reflected vision. Of mirrors and images reflected from them. Of refracted vision, with the nature of lenses and images exhibited thereby. Of burning glasses. Description of the eye. Manner of vision. Of long and short-sighted eyes. Of spectacles, telescopes, and microscopes. Of the rainbow.

"*Sixth and Seventh — Pneumatics.*

"Of the atmosphere. The elasticity of the air. Description of the air-pump. The spring and weight of the air proved by a great variety of experiments on the air-pump. Of respiration. Of sound. Of winds. Of the blueness of the sky. Of twilight.

"*Eighth, Ninth, and Tenth — Astronomy.*

"Introduction. Of the solar system. Of the figures, magnitudes, distances, motion, etc., of the sun, planets, and comets. Of the progressive motion of light. Of the fixed stars and their phenomena. Of the lunar planets. Of eclipses, tides, etc.

"*Eleventh and Twelfth — Use of the Globes.*

"Figure of the earth. Description of the globes. Various problems performed thereon, amongst which are an explanation of the phenomena of the harvest-moon and the variations of the seasons. Conclusion.

"'Ex rerum causis supremam noscere causam.'"

Miss Johns, whose diary will be afterwards referred to, tells us that this very syllabus, and one for 1792, came accidentally in Dalton's way in

after-life, when he was looking over some old letters, having been detained in the house by a cold. He burst out into a loud laugh.

In the subsequent years of his life at Kendal, Dalton was in the habit of repeating these lectures, both in that town and elsewhere, but with, as it would seem, but scanty success; for in 1791 he found it desirable to lower the admission fee to one-half of what he charged in 1787. Nor is this much to be wondered at; for if, as was the case, the verdict given of his powers as a popular expositor of science in after-years, when he had had more experience and knowledge, was unfavourable (*see* p. 163), it is reasonable to imagine that only a few out of a mixed audience of Kendal folk of that day would appreciate, hidden under the rough and uncouth demeanour of the lecturer, the true value of the scientific information which he was able to impart.

Dalton, at this time, also devoted some attention to Botanical Classification and to collecting the flora of the district. Thus he hoped again to earn an honest penny; for he writes to Mr. Crosthwaite, of Keswick, that he has dried and pressed a good many plants and pasted them down to sheets of white paper, and found that they look very pretty, and attract the attention of the learned and unlearned, and that a tolerable collection of them, treated in this manner, would be a very proper object for the Keswick Museum; and, he adds, that he

thinks he could engage to fill a book of two quires for half a guinea. Whether Mr. Crosthwaite obtained any sale for these collections is not recorded; but Dalton must have laboured long and pretty successfully in collecting the Kendal flora, as we find that his complete "Herbarium" occupied eleven volumes, of which the title-page of the first, dated 1790, ran:—"Hortus siccus, seu Plantarum diversarum in Agris Kendal vicinis sponte nascentium Specimina, Opere et Studio Johannis Dalton collecta, et Secundum Classes et Ordines disposita." More than a collector Dalton was not; for Scientific Botany as we now know it was then non-existent.

From collecting the plants of the district, Dalton soon passed on to collecting the commoner insects; and his butterflies and moths were long to be seen iu the Keswick Museum. "Some of these may be thought puerile; but nothing," he writes to Crosthwaite, "that enjoys animal life, or that vegetates, is beneath the dignity of a naturalist to examine." The desire for knowledge—the strongest motive of his life—induced him to inquire into the limiting conditions of existence amongst the lower animals; and he made experiments to ascertain how far the vitality of snails, mites, and maggots was destroyed or suspended by immersion in water or by being placed in a vacuum. Next, he began to experiment upon himself—his idea being that quantitative estimations of the food taken

in, and of the ejecta thrown out by the body, would yield the amount of insensible perspiration. Forty years afterwards he published the numerical results of these experiments. He there states that he had some intention of beginning the study of medicine, "with a view to future practice; and that on that account, but also partly from my own personal interest in knowing the cause of disease and of health, that I was prompted to make such investigations into the animal economy as my circumstances and situation at the time would allow." So far was this wish to take up medicine as a profession at one time a fixed idea in his mind that he consulted many friends on the subject; but they advised him to give up the project. Elihu Robinson, who fully appreciated his friend's original powers, writes: — "As I have thought thy talents were well adapted to thy present profession, I cannot say thy proposal of changing it was very welcome to me; believing thou wouldst not only shine, but be really useful in that noble work of teaching youth." As Dr. Henry says, the world has no cause to regret Dalton's decision; for it may be taken for granted that if he had become a country practitioner, his energies would have been thrown into other channels, and science would have lost one of its most original and powerful votaries.

A new era for Dalton was now about to open. In 1793 he left Keswick and came to Manchester.

CHAPTER II.

EARLY YEARS IN MANCHESTER.

As Dalton in 1793 became connected with Manchester—now a university city—as a teacher in an academy or college in which the higher education was given, it may not be out of place here to remark that several proposals have been made in past times to found a university in this part of the kingdom. So long ago as 1640 Henry Fairfax wrote to his brother Fernandino, the second Lord Fairfax, enclosing a memorial from the public of Manchester praying the Long Parliament to grant a charter to a northern university, adding, "Posterity may bless you, and the work will speak for itself that the like hath not been in England (if Cambridge be the last) not of two thousand years." The Knights of the Shires for Lancashire and Cheshire being consulted by Fairfax on the matter, "he found them hopeless of having it. The way to effect it must be by bill, which will be a charge of 100 marks at least (£66 13s. 4d.), and therefore I think it fittest to let that rest, and let none come to solicit it in this troublesome

time, when all businesses of the commonweal are at a stay, my Lord of Strafford still keeping us in play." Troublesome times pass over; but these are not the only difficulties which beset the founding of new institutions; for when Cromwell had leisure to propose a Member of Parliament for Manchester, and to give ear to the other desires of the town, rival claims sprang up in York; the Wars of the Roses were again waged, this time on educational grounds, the upshot being that Cromwell gave to neither the university which each desired, but by sequestrating the funds of the Dean and Chapter founded the University of Durham. In 1789, and again in 1836, similar proposals were made; but the citizens of Manchester had to wait for the accomplishment of their wishes until the Victorian era (1880), when a Royal Charter constituting the Victoria University was granted by Her Majesty.

The college in which Dalton became tutor in mathematics and natural philosophy was one established by the Presbyterians of Manchester as a continuation of the Warrington Academy, with which men such as Priestley, Aikin, Enfield, and Reinhold Forster had been honourably connected. The object of this institution was to give a high-class education, free from every religious or political test, both to laymen, and to candidates for the Presbyterian ministry. Its foundation was a protest against the religious exclusiveness of the English universities at

that date — for then neither to Unitarians, however eminent, like Joseph Priestley, or to Quakers, such as Thomas Young or John Dalton, were the doors of Oxford or of Cambridge open. If Manchester New College had never done anything else — and the fact that Martineau is an alumnus of the college is a guarantee that it has done much more — than to give scope to the genius of Dalton, and thus enable him to rise from the drudgery of a village schoolmaster to do his great work for the world, the college would have deserved well of the nation.*

It was through Mr. Gough that Dr. Barnes, the principal of the college, heard of Dalton. The post was far from being a lucrative one. The guaranteed salary was £80 for the session, lasting from September to June, out of which sum £27 10s. had to be paid to the college for commons and rooms. This left Dalton only with £50 a year; but this was a larger sum than he had ever yet had placed at his disposal, and so he remained connected with the Manchester Academy for six years.

* The "Academy" remained in Manchester until 1803, when it was removed to York, but in 1840 again returned to its original site in Manchester. In 1889 "Manchester College" was again removed, this time to Oxford, where stately buildings have been erected next to Mansfield College. Over the entrance to the main building stands the inscription, "To Truth! To Liberty! To Religion!" being the words with which a discourse entitled "Free Teaching and Free Learning" was concluded by the first principal of the college, Dr. Thomas Barnes, D.D., in the inaugural address delivered on September 14th, 1786.

Knowing what Dalton became, the following report of the college trustees for 1797 on his conduct and capabilities is rather amusing:—"In the province of mathematics, natural philosophy, and chemistry, Mr. Dalton has uniformly acquitted himself to the entire satisfaction of the trustees, and has been happy in possessing the respect and attachment of his pupils."

In 1794 Dalton had twenty-four pupils in mathematics, mechanics, geometry, book-keeping, natural philosophy, and chemistry. He used Lavoisier's "Elements of Chemistry" and Chaptal's "Chemistry" amongst others.

The following letter to Robinson, descriptive of life in Manchester a century ago, is worthy of record:—

"MANCHESTER, 2 mo., 20th, 1794.

"DEAR COUSIN,—Amidst an increasing variety of pursuits, amidst the abstruse and multifarious speculations resulting from my profession, together with frequent engagements to new friends and acquaintance, shall I find a vacant hour to inform thee where I am and what I am doing? Yes; certainly one hour of sixteen some day may be spared for the purpose.

"I need not inform thee that Manchester *was* a large and flourishing place. Our academy is a large and elegant building in the most elegant and retired street* of the place. It consists of a front and two wings; the first floor of the front is the hall, where most of the business is done; over it is a library, with about 3,000 volumes; over this are two rooms, one of which is mine; it is about eight yards by six, and above three high, has two windows

* Manchester men of the present day will not recognise Mosley Street in this description.

and a fire-place, is handsomely papered, light, airy, and retired; whether it is that philosophers like to approach as near to the stars as they can, or that they choose to soar above the vulgar into a purer region of the atmosphere, I know not; but my apartment is full ten yards above the surface of the earth. One of the wings is occupied by Dr. Barnes' family; he is one of the tutors, and superintendent of the seminary; the other is occupied by a family who manage the boarding, and seventeen in-students with two tutors, each individual having a separate room, etc. Our out-students from the town and neighbourhood at present amount to nine, which is as great a number as has been since the institution; they are of all religious professions; one Friend's (Quaker) son from the town has entered since I came. The tutors are all Dissenters. Terms for in-students, 40 guineas per session (10 months); out-students, 12 guineas. Two tutors and the in-students all dine, etc., together in a room on purpose. We breakfast on tea at 8½, dine at 1½, drink tea at 5, and sup at 8½; we fare as well as it is possible for any one to do. At a small extra expense we can have any friend to dine with us in our respective rooms. My official department of tutor only requires my attendance upon students 21 hours in the week; but I find it often expedient to prepare my lectures previously.

"There is in this town a large library, furnished with the best books in every art, science, and language, which is open to all gratis; when thou art apprised of this and such-like circumstances, thou considerest me in my private apartments, undisturbed, having a good fire, and a philosophical apparatus around me, thou wilt be able to form an opinion whether I spend my time in slothful inactivity of body and mind. The watchword for my retiring to rest is 'Past—12 o'clock—cloudy morning' . . .

"There is a considerable body of Friends (Quakers) here; near 200 attend our first day (Sunday) meetings. I have received particular civility from most of them, and am often at a loss where to drink tea on a first-day afternoon, being pressed on so many hands. One first-day lately I took a walk in company with another to Stock-

port; there are but few Friends there, but the most elegant little meeting-house that can be conceived; the walls and ceiling perfectly white; the wainscot, seats, gallery, etc., all white as possible; the gallery-rail turned off at each end in fine serpentine form; a white chandelier; the floor as smooth as a mahogany table, and covered with a light-red sand; the house well lighted, and in as neat order as possible; it stands on a hill; in short, in a fine sunny day it is too brilliant an object to be attended, by a stranger at least, with the composure required.

"JOHN DALTON."

In this position of College Tutor Dalton remained, as has been said, for six years, when he resigned his post and devoted himself to the prosecution of scientific inquiry, but earning his bread as a private teacher, principally in Mathematics and Natural Philosophy, to such as might come to him, at a charge of two shillings a lesson.

In the year 1826, when Dalton had achieved a European renown, M. Pelletier, a well-known Parisian *savant*, came to Manchester with the express purpose of visiting the illustrious author of the Atomic Theory. Doubtless, he expected to find the philosopher well-known and appreciated by his fellow-citizens—probably occupying an official dwelling in a large national building devoted to the prosecution of science, resembling, possibly, his own Collège de France or Sorbonne. There he would expect to find the great chemist lecturing to a large and appreciative audience of advanced students.

What was the surprise of the Frenchman to find, on his arrival in Cottonopolis, that the whereabouts of Dalton could only be found after diligent search; and that, when at last he discovered the Manchester philosopher, he found him in a small room of a house in a back street, engaged looking over the shoulders of a small boy who was working his "cyphering" on a slate. "Est-que j'ai l'honneur de m'addresser à M. Dalton?" for he could hardly believe his eyes that this was the chemist of European fame, teaching a boy his first four rules. "Yes," said the matter-of-fact Quaker. "Wilt thou sit down whilst I put this lad right about his arithmetic?"

On December 15th, 1797, he writes to his brother: "My time at present is much taken up with tuition at home and in the town together; so that I can scarcely turn to any particular and mathematical or philosophical pursuit; but of late I have been attending to the philosophy of grammar, and to that of sound." Again, in 1801, he describes his occupations:—"Since the year came in, I have not been much troubled with *l'ennui*. Eight regular pupils by day, and as many more in the evenings, to whom I have sometimes given fifteen lessons a week; my grammar in the press; the whole of it to write over and to retouch, and to attend to the press; have required a considerable activity of both body and mind." Each of his day pupils paid him ten guineas per annum, and the others two shillings a lesson. "I am not yet

rich enough to retire, notwithstanding," he writes to Elihu Robinson in 1802.

Dalton doubtless gave instruction in other subjects than mathematics or physics; and that his interests were of a wider character we may certainly infer from the fact, to which he refers in the above letter, that he wrote an English Grammar, which was published in 1801. Dalton greatly admired the writings of Horne Tooke; and to Tooke he dedicated his Grammar, and acknowledges in the following fashion his indebtedness to the author of the "Diversions of Purley": — "To the literary world it will be necessary to observe that in this department, Etymology, I have drawn a great deal from one source; but I have not rested satisfied with the *ipse dixit* of the 'Diversions of Purley,' when time and opportunity afford me means of confirmation and inquiry."

He sent a copy of his Grammar to Horne Tooke, as he writes to Jonathan, and then adds: — "But he has got *things* to attend to now instead of *words*" — alluding to the fact of the arraignment of Tooke on a charge of high treason.

The Grammar appears not to have been devoid of merit; for one thing, he gets rid of "articles" from the parts of speech, and classes them with adjectives as "definitives." His view of "tense" was that of a man of science. "It may be taken as an axiom that all time or duration, in the strict sense of the terms, is either past or future. But for the purposes of

speech, we must have a present time of some duration, which must necessarily be comprised of a portion of past and a portion of future — having the present, *now* or *instant*, as a boundary between them. Its length may be what we please to make it. Grammatically speaking, therefore, there are three times — past, present, and future; though, strictly and mathematically speaking, we can admit only two, past and future." He made one rather singular mistake when he states, among the three ways of distinguishing the sexes in English, there is one by change of termination, and gave as examples: "prince — princess, phenomenon — phenomena."

The book does not seem to have enjoyed a very extended sale, although it appears from a letter to his brother, dated 11th of November, 1803, a second edition was called for. On going some years afterwards to the shop of his publisher to ask for a copy of his Grammar, he was told that they had none left. On a more careful search being made, a large parcel of the book was found in a dusty corner, very few having been sold. Dalton himself, however, used to say that some man in Sheffield found it so much to his mind that he published it some years afterwards as his own, with additions.

When he arrived in Manchester, Dalton brought with him the manuscript of a volume entitled "Meteorological Observations and Essays," and this was printed and published in 1793, and appeared in

a second and enlarged edition forty years later. These observations were made and the book written at Kendal. His friends Robinson and Gough had set him the example of carrying out weather observations, and the varying climate of the uplands on which he was born, and the twenty-six years spent amongst the fells and streams of the Lake district, led him insensibly to the study of meteorological phenomena. The splendid appearances of the aurora, more frequently witnessed in that district than in southern England, also stimulated his interest. Indeed, the first entry in his record was that of an aurora seen on March 27th, 1787. "In the evening," he writes, "soon after sunset, there appeared a remarkable aurora borealis, the sky being generally clear and the moon shining; it spread over one-half of the hemisphere, appeared very vivid, and had a quick vibrating motion."

The volume above referred to contains a long description of auroral phenomena, and an investigation as to their cause. These remarks are characteristic of Dalton's independence, and of the neglect—or contempt, one might even say—with which he treated the views of others—a failing not uncommon amongst self-educated but original minds. This independence of spirit and reliance on his own work was clearly expressed by Dalton in the preface to his great work, "The New System of Chemical Philosophy," of which more will be said hereafter.

In the preface to the second part, published in 1810, he says: "Having been in my progress so often misled by taking for granted the results of others, I have determined to write as little as possible but what I can attest by my own experience." He begins his essay on the aurora as follows: "As this essay contains an original discovery which seems to open out a new field of inquiry in philosophy, or rather, perhaps, to extend the bounds of one that has been as yet just opened, it may not perhaps be unacceptable to many readers to state briefly the train of circumstances which led the author to the important conclusions contained in the following pages."

These conclusions related to the connection between auroral displays and the earth's magnetism, and to the height of the auroral arch above the earth's surface. Before his time the action of the aurora upon the magnetic needle had been pointed out by Wargentin, Celsius, and Halley. The latter supposed the aurora to be caused by magnetism. Dalton, probably ignorant of what had already been done, does not mention the names of previous workers. He decides that the phenomena are closely connected with magnetism. The very grand aurora of October 13th, 1792, was that which first suggested to him the relation between the phenomena and the earth's magnetism.

"When the theodolite was adjusted without doors, and the needle at rest, it was next to impossible not to notice the exactitude with which the needle pointed to the middle of the concentric arches. Soon after the grand dome being formed it was divided so evidently into two similar parts by the plane of the magnetic meridian that the circumstances seemed extremely improbable to be fortuitous; and a line drawn to the vertex of the dome, being in the direction of the 'dipping needle,' it followed, from what had been done before, *that the luminous beams at that time were all parallel to the 'dipping needle.'* It was easily and readily recollected at the same time that former appearances had been similar to the present in this respect, and that the beams to the east and west had always appeared to decline considerably from the perpendicular towards the south, whilst those to the north and south pointed directly upwards. The inference, therefore, was unavoidable that the beams were guided not by gravity, but by the earth's 'magnetism' and the disturbance of the needle that had been heretofore observed during the time of an aurora seemed to put the conclusion past doubt.

"It was proper, however, to observe whether future appearances corresponded thereto, and this has been found invariably the case, as related in these observations."

Then comes Dalton's somewhat daring assumption, not, however, borne out by the most recent spectroscopic research: "Now, from the conclusions in the preceding sections, we are under the necessity of considering the *beams* of the *aurora borealis* of a *ferruginous* nature, because nothing else is known to be magnetic, and, consequently, that there exists in the higher regions of the atmosphere an elastic fluid partaking of the properties of *iron*, or rather of *magnetic steel.*" Since Dalton's

day science has been provided with the means of determining, by help of the spectroscope, the chemical constitution of the atmospheres of the sun and of the far distant fixed stars. Nor has the light from other self-luminous cosmic material, such as nebulæ and comets, escaped the analytical power of the prism. The auroral light, too, has also been made to tell its own tale, with the result that a bright green line is always found in its spectrum; and the point of interest to us here is that this line is not found to be coincident with any of the well-known lines in the spectrum of metallic iron. This riddle of the heavens has not yet been properly read. The origin of this green line is, as yet, not ascertained, as it does not belong to any known substance. It is, however, interesting to learn that it is also seen in the spectrum of the Zodiacal light.

No point respecting the aurora has been more keenly discussed, nor the results of the discussion been more widely divergent, than that of its height. The estimates vary from feet to thousands of miles. Dalton at first believed the height to be about a hundred miles; but later, in 1834, he describes an auroral arch, of which he calculates the height from a base line drawn from Birmingham to Manchester, to be probably six or eight hundred miles from the earth, "being far beyond the height calculated by Mr. Cavendish of that which was seen at Kimbolton, near Cambridge."

He adheres to his former opinion on the theory of the display. "I have for the last forty years," he says, "considered both arches and beams to be constituted of *magnetic* matter, and in ordinary circumstances invisible; but when a disturbance of the electric fluid takes place in the upper regions these beams, etc., serve to convey the electric fluid from one place to another to restore the equilibrium, which occasions the luminous appearances."

In the preface to these Meteorological Essays Dalton says that, in collecting information, he had not "a superabundant assistance from books," but relied on his own observations. This absence of knowledge of the history of his subject, to which reference has already been made, sometimes placed him in difficulties. Thus his theory of the trade winds — viz., that they were caused by the earth's rotation — had long before been explained and published in the Philosophical Transactions for 1735 by George Hadley. "The inequality of heat," writes Dalton, "in the different climates and places, and the earth's rotation on its axis, appear to me to be the grand and chief causes of winds, both regular and irregular, in comparison with which all the rest are trifling and insignificant."

This statement appears to contain "the truth, the whole truth, and nothing but the truth," so far as we now know. For Maury, James Thomson, and other modern authorities admit its validity, whilst

the results of other observers — such as those of Dove known as his law of storms — certainly do not conflict with this more general law of atmospheric disturbance.

Again, after publishing his conclusions respecting the variation of the barometer, he discovered that he had been forestalled by De Luc. This he duly acknowledged, adding that he considered it to be a favourable circumstance for any theory when it is deduced from a consideration of facts by two persons independently of each other.

The essays on the barometer, on the thermometer and their variations, on the hygrometer, on the aurora, on rainfall, on the formation of cloud, on evaporation, on the distribution and character of atmospheric moisture, written, as they were, at the end of last century, might well be considered as remarkable productions from the pen of an experienced philosopher. How much more remarkable must they appear when we remember that they were written by a young Kendal schoolmaster ignorant to a great extent of what had been written by others, and out of reach of libraries and of books of reference.

Perhaps the most pregnant of his conclusions contained in these essays, as leading on to others of still greater consequence, was that on Evaporation. After relating his experiments to ascertain for himself the variation of the boiling-point of water with the varying pressure of the air, he says:

"Upon the consideration of the facts it appears to me that evaporation and the condensation of vapour in the atmosphere are not the effect of chemical affinities, but that aqueous vapour always exists as a fluid *sui generis*, diffused amongst the rest of the aërial fluids." Moreover, as a certain definite quantity of vapour of water is imbibed by every cubic foot of air, if the temperature be the same, the quantity of moisture which a given bulk of air can take up at a given temperature can be calculated. This observation lies at the root of the law now known as "Dalton's law of vapours" —which is expressed by saying that the same weight of moisture is taken up by a given space, at a given temperature, whether that space be filled with air or be destitute of air. Since Dalton's time, exact experiment has proved that about 2 per cent. more moisture is taken up *in vacuo* than in the air; but possibly this difference is due to the hygroscopic character of the walls of the chamber, and, if so, Dalton's law is true after all.

Although Dalton was not the first to suggest that rain is caused, not by any alteration in atmospheric pressure, but simply and solely by a diminution of temperature, he was the first to render this conclusion certain. When moist air is cooled below the dew-point, the aqueous vapour is deposited in the form of drops, which, if very small, remain suspended in the air as cloud, or, if larger, fall as rain. The

weight of rain which thus falls is enormous; if we suppose a cubic mile of air — a minute fraction of the air above any locality — to be saturated at a temperature of 95° Fahr., and then that this temperature be reduced to the freezing-point (conditions which constantly happen), no less than 140,000 tons of water will fall as rain.

These conclusions of Dalton were of importance. Up to that time it had not been possible to ascertain the amount of atmospheric deposit due to a given lowering of temperature; by following them this could be determined by a simple observation of the dew-point. Dalton's method of ascertaining this point was very different from that now employed. At the present day the finest and most delicate thermometers are used; the exact temperature at which the dew is deposited is ascertained by the bedimming of a gilt ring or of a silver thimble, the ring or thimble being cooled by the evaporation of ether. Dalton used rough home-made thermometers, and the method of carrying out the experiment was so characteristic of the man, that it can only be fully appreciated in his own words: —

"The usual mode of my operations was to find a spring on the side of the mountain and then to take a cup of water from it and pour into a clear dry tumbler glass. If dew was produced immediately on the outside of the glass, the water was returned into the cup, and the glass was again carefully dried outside. During this time the water in the cup was acquiring temperature from the air.

It was then returned into the tumbler and held out exposed to the current of air. This process was repeated till no dew was found to be formed on the glass. The temperature of the water each time it was put into the tumbler was found by a small pocket thermometer; and *that* when it last produced dew on the glass was marked down as the dew-point. At the same time the barometer was noted to find the height of the place of observation, and the thermometer to find the temperature of the air; and the temperatnre of the springs was an object not wholly devoid of interest. Difficulties, however, sometimes occurred. Springs were not always to be found where they were wanted, and many times when found the water was not cold enough to produce dew. In such cases a teaspoonful or two of powdered nitre and sal-ammoniac are thrown into the water and stirred about till dissolved. This generally succeeded. On two or three occasions large *snowdrifts* were found on the north-east side of the mountain summit, which, being accessible, were particularly useful for the purpose of reducing the temperature of the water, especially as it was easy to carry a quantity of it in a basket and preserve it for a day or more."

With regard to another subject which is attracting much attention at present, Dalton's observations are worthy of notice. We now know that all gases, including probably even hydrogen, by far the most refractory gas, can be liquefied, and even solidified. It is interesting to remember that a prediction to this effect was made by Dalton long before Faraday had published his first experiments on the condensation of the so-called permanent gases in 1821. "There can scarcely be a doubt entertained," he says, "respecting the reducibility of all elastic fluids of whatever kind into liquids; and we ought not to despair

of effecting it in low temperatures, and by strong pressure exerted upon the unmixed gases."

Although in all his experiments Dalton made use of rough apparatus — very different from that now necessary for physical or chemical research — it is interesting to find how nearly many of his numerical results approach those since obtained by much more careful work, and by infinitely more accurate methods and instruments. Moreover, Dalton was fully aware of the existence of the experimental errors which his processes involved. Thus with regard to the modes then adopted for the determination of the hygrometrical condition of the air, he remarks in 1793 that "to ascertain the exact quantity of water in a given quantity of air is, I presume, an object not yet fully attained." Again in 1799 he determined the point of maximum density of water to be 38° Fahr. Some years afterwards Playfair and Joule corrected this number to 39·1°. His mode of arriving at results was, however, always ingenious, though often rough; indeed, as Angus Smith says, he seems to have begun his experiments with his hands and finished them off with his head.

As an illustration of this, we may quote, in his own words, a description of an experiment made in 1799 to confute Count Rumford's conclusion that liquids "are perfect non-conductors of heat." "Took an ale-glass of a conical figure, two and a half inches in diameter and three inches deep; filled it with

water that had been standing in the room, and consequently of the temperature of the air nearly, put the bulb of the thermometer to the bottom of the glass, the scale being out of the water. Then having marked the temperature, I put the red-hot tip of the poker half an inch deep in the water, holding it there steadily about half a minute; and, as soon as it was withdrawn, I dipt the bulb of a sensible thermometer into the water, when it rose in a few seconds to 180°.

"TEMPERATURE.

Time.	At Top.	Middle.	Bottom.
Before the poker was immersed			47°
——	180°	—	47°
5 min.	100°	60°	47½°
20 ,,	70°	60°	49°
1 hour	55°	—	52°

"These experiments all evidently agree in proving water to have a proper conducting power, independent of any internal motion. It surely will not be said that any slight motion unavoidably made at the beginning of an experiment could continue with a powerful effect for upwards of an hour. However, to determine this matter, I made the two following experiments." After describing several, all of which give the same result, he remarks: "We must conclude, therefore, that the quick circulation of heat

in water over a fire, etc., is owing *principally* to the internal motion excited by an alteration of specific gravity; but not *solely* to that cause, as Count Rumford has inferred."

On June 27th, 1800, he read an important paper "On the Heat and Cold produced by the Mechanical Condensation and Rarefaction of Air." In this for the first time he measured the amount of heat evolved by compression and heat absorbed by dilatation — determinations which, repeated forty years afterwards by Mayer, of Heilbronn, led to what was subsequently proved by Joule to be the true mechanical equivalent of heat. Of this equivalency of heat and mechanical energy Dalton had, it appears, no idea, nor was his estimation anything more than a rough approximation; but it was a beginning, and perhaps the reading of this essay may have led Joule to consider the probable existence of such an equivalence, and may have induced him to make the experiment on a sounder basis.

Five years elapsed before he again communicated a memoir to the Philosophical Society, of which he became secretary in 1800, a vice-president in 1808 (in place of Dr. Roget, who up to that time had resided in Manchester), and president in 1817, an honourable office which he retained until his death in 1844. During this period of apparent rest, Dalton's mind and hands were doubtless busy with

preparation for the long series of researches which he gave to the world in the next few years.

This, the most prolific period of his life, will be discussed in the following chapters; but before passing to this more serious matter it may be well to learn something about his peculiar eyesight.

CHAPTER III.

DALTON'S COLOUR-BLINDNESS.

The story goes that, desiring to give his mother a birthday present, John Dalton took her a pair of stockings which he saw in a shop window in Kendal, labelled, "Silk, and newest fashion." This would be something fresh for Deborah, who was accustomed to wear her own home-made, knitted, drab-coloured woollen hose. "Thou has bought me a pair of grand hose, John, but what made thee fancy such a bright colour? Why, I can never show myself at meeting in them." Her son, much disconcerted by this exclamation, told her that to his eyes these stockings were of a dark-bluish drab, a very proper sort of go-to-meeting colour. "Why, they're as red as a cherry, John." Neither he nor his brother Jonathan could see anything else than drab in these scarlet stockings, and they both came to the conclusion that the old lady's sight was strangely out of order, until Deborah, having consulted neighbouring wives on this singular difference of opinion, returned with the reply, "Varra fine stuff, but uncommon scarlety." This was the first event which opened John Dalton's

eyes to the fact that his (and his brother's) vision was not as other men's. It is, indeed, stated that before the year 1793, when he made, as we shall learn, a precise investigation of this peculiarity in his eyesight, both he and Jonathan tested the vision of their scholars, in several of whom they proved the existence of a similar deficiency in the power of recognising colour. It seems almost incredible that Dalton should have lived to the age of twenty-six without noticing this peculiarity; and yet it appears less strange when we remember that thousands of persons have, probably since time was, suffered in a similar manner without knowledge of their infirmity; and, indeed, Dalton was almost the first to direct the attention of the scientific world to the subject.

The first communication Dalton ever made to the Literary and Philosophical Society of Manchester was a paper read on October 31st, 1794,* entitled, "Extraordinary Facts relating to the Vision of Colours: with Observations by Mr. John Dalton," thus commencing a series of important memoirs to the Society only ending with his death.

In February of that year he wrote a letter to his "dear cousin," Elihu Robinson, describing his own colour sensations, in which he remarks: "I was the other day at a friend's house who is

* Dalton was elected a member of the Society on October 3rd, 1794, having been proposed by Dr. Thomas Henry, Dr. Percival, and Robert Owen, the veteran social reformer.

a dyer; there was present himself and wife, a physician, and a young woman. His wife brought me a piece of cloth; I said I was there in a coat just of the colour a few weeks before, which I called a reddish snuff colour. They told me they have never seen me in any such coat, for that cloth was one of the finest grass-greens they had seen. I saw nothing like grass about it. They tell me my tablecloth is green, but I say not, and that I never saw a green tablecloth in my life but one, and everybody said it had lost its *green* colour. In short, my observations have afforded a diversion to all, and something more to philosophers, for they have been puzzled beyond measure, as well as myself, to account for the circumstances. I mean to communicate my observations to the world through the channel of some philosophical society. The young women tell me they will never suffer me to go into the gallery (of the Meeting-house) with a *green* coat; and I tell them I have no objection to their going on with me in a crimson (that is, dark drab) gown."

Many years after this, when Dalton was about to visit Paris for the purpose of making the acquaintance of the French *savants* — wishing, even as a Quaker, to put in a respectable appearance for the occasion—he went to a tailor's in Manchester (Dalton was at that time a well-known character in that city) and said, "I am going to Paris; I want thee to sell me some good stout drab cloth," and passing his

hand over a piece lying on the counter, he remarked, "I think this will suit; just the colour I want, and stout good cloth." "Why," said the tailor, "Dr. Dalton, that is a piece of scarlet cloth for hunting-coats!" to which the Doctor replied, "Ah! I see thou knowest the infirmity of my eyes."

Continuing his investigations, Dalton came across a letter addressed to Priestley, and printed in the Philosophical Transactions for 1777 by Captain Joseph Huddart, containing a description of the abnormal eyesight of some members of a family named Harris, of Maryport. This description did not satisfy Dalton, and so he writes on "3rd, 10th month, 1793," to "Respected Friend, Joseph Dickinson," of the above town, requesting him, as an intelligent son of Crispin, to put a series of distinct questions to the sea-captain whose peculiar vision was reported upon by Huddart. The questions were duly put and answered. Two of the Harrises — "Thos. and the captain thou mentioned — are dead long ago," and therefore their replies are not forthcoming; but two other brothers were living, and their eyesight was examined and reported upon, for, singularly enough, as it turned out, four of these brothers Harris were "colour-blind." Yellow was the most conspicuous colour which they saw in the solar spectrum. Roses, pinks, etc., which we call red, appear to them to have some affinity to sky-blue. A tablecloth of Kendal green don't appear like grass; red sealing-wax appears rather darker;

and there is no difference between dark green and blood. Wishing one of them to choose out a coloured worsted nearest resembling blood, to Crispin's astonishment, Harris chose out a dark green. Asked if a white cloth were spotted with blood he could perceive it, he said he would not know it from dirt. Asked whether he ever saw blood near slaughter-houses, he replied that he had perceived a wetness, and judged it to be blood from the little bells of froth frequently upon it, which was all he knew it by.

Dalton seems to have been satisfied by this information that the friends Harris's eyes were constituted like those of his own and his brother. Replying to Dickinson, he writes: "It is a subject that has not been much handled by philosophers; I mean, therefore, to make inquiries in different places, to ascertain the facts as well as I can, and then endeavour to account for them. The result of my labours will be communicated to the public in some way or other." The friends Harris were, it seems, rather backward in giving their judgment, but Dalton does not wonder at it. "But tell them," he writes, "that formerly, when I used to call pink sky-blue, and incur the ridicule of others, I used to join in the laugh myself, and then nobody thought I was in earnest; nor did I think at that time that there was such a great difference in the appearance of colour to me and to others as there now seems there is. I thought we differed chiefly in words, and not ideas;

but now I see that I am certain of a real and very great difference, and I boldly assert with a grave face that pinks and roses are light blue by day and a reddish yellow by night, that crimson is a bluish dark drab, that all greens (so miscalled) are of a red or blood colour, and the most disagreeable colour imaginable for a table—infinitely different from the pleasant verdure of the fields." In a further letter, the Maryport shoemaker chaffs John on his infirmity very neatly, as follows:—"I find by your accounts you must have very imperfect ideas of the charms which in a great measure constitute beauty in the female sex: I mean that rosy blush of the cheeks which you so much admire for being light blue—I think a complexion nearly as exceptional in the fair sex as the sunburnt Moor's or the sable Ethiopian's, consequently (if real) a fitter object for a show than for a wife."

To give the reader an idea of Dalton's scientific style—which, like the man, is simple enough, not beating about the bush, but going straight to the point—it may be of interest to quote a few extracts from the Memoir in which he gives his promised investigation of the subject.

"EXTRAORDINARY FACTS

RELATING TO THE

VISION OF COLOURS,

WITH OBSERVATIONS

BY MR. JOHN DALTON.

"It has been observed that our ideas of colours, sounds, taste, etc., excited by the same object may be very different

in themselves without our being aware of it; and that we may, nevertheless, converse intelligibly concerning such objects as if we were certain the impressions made by them on our minds were exactly similar. All, indeed, that is required for this purpose is that the same object should uniformly make the same impression on each mind, and that objects which appear different to one should be equally so to others. It will, however, scarcely be supposed that any two objects which are every day before us should appear hardly distinguishable to one person and very different to another without the circumstance immediately suggesting a difference in their faculties of vision; yet such is the fact, not only with regard to myself, but to many others also, as will appear in the following account.

"I was always of opinion—though I might not often mention it—that several colours were injudiciously named. The term *pink*, in reference to the flower of that name, seemed proper enough; but when the term *red* was substituted for pink I thought it highly improper. It should have been *blue*, in my apprehension, as pink and blue appear to me very nearly allied, whilst pink and red have scarcely any relation.

"In the course of my application to the sciences that of optics necessarily claimed attention, and I became pretty well acquainted with the theory of light and colours before I was apprised of any peculiarity in my vision. I had not, however, attended much to the practical discrimination of colours, owing, in some degree, to what I conceived to be a perplexity in the nomenclature. Since the year 1790 the occasional study of botany obliged me to attend more to colours than before. With respect to colours that were *white*, *yellow*, or *green*, I readily assented to the appropriate term. *Blue*, *purple*, *pink*, and *crimson* appeared rather less distinguishable, being, according to my idea, all referable to *blue*. I have often seriously asked a person whether a flower was blue or pink, but was generally considered to be in jest. Notwithstanding this, I was never convinced of a peculiarity in my vision till I accidentally observed the colour of the flower of the *Geranium zonale* by candlelight in the autumn of 1792. The flower was pink, but it ap-

peared to me almost an exact sky-blue by day; in candlelight, however, it was astonishingly changed, not having then any blue in it, but being what I called red—a colour which forms a striking contrast to blue. Not then doubting but that the change of colour would be equal to all, I requested some of my friends to observe the phenomenon, when I was surprised to find they all agreed that the colour was not materially different from what it was by daylight except my brother, who saw it in the same light as myself. This observation clearly proved that my vision was not like that of other persons, and, at the same time, that the difference between daylight and candlelight on some colours was indefinitely more perceptible to me than to others. It was nearly two years after that time when I entered upon an investigation of the subject, having procured the assistance of a friend who to his acquaintance with the theory of colours joins a practical knowledge of their names and constitutions. I shall now proceed to state the facts ascertained under the three following heads:—

"I.—An account of my own vision.

"II.—An account of others whose vision has been found similar to mine.

"III.—Observations on the probable cause of our anomalous vision.

"I.—Of My Own Vision.

"It may be proper to observe that I am short-sighted. Concave glasses of about five inches focus suit me best. I can see distinctly at a proper distance, and am seldom hurt by too much or too little light, nor yet with long application.

"My observations began with the spectrum, or coloured image of the sun, exhibited in a dark room by means of a glass prism. I found that persons in general distinguish six kinds of colour in the solar image—namely, red, orange, yellow, green, blue, and purple. My yellow comprehends the red, orange, yellow, and green of others; and

my blue and purple coincides with theirs. That part of the image which others call red appears to me little more than a shade, or defect of light; after that, the orange, yellow, and green seem one colour, which descends pretty uniformly from an intense to a rare yellow, making what I call different shades of yellow. The difference between the green part and the blue part is very striking to my eye: they seem to be strongly contrasted. That between the blue and purple is much less so. The purple appears to be blue much darkened and condensed. In viewing the flame of a candle by night through the prism the appearances are pretty much the same, except that the red extremity of the image appears more vivid than that of the solar image.

"I now proceed to state the results of my observations on the colours of bodies in general, whether natural or artificial, both by daylight and candlelight. I mostly used ribands for the artificial colours.

"*Red (by Daylight).*

"Under this head I include *crimson*, *scarlet*, *red*, and *pink*. All crimsons appear to me to consist chiefly of dark blue; but many of them seem to have a strong tinge of dark brown. I have seen specimens of *crimson*, *claret*, and *mud* which were very nearly alike. Crimson has a *grave* appearance, being the reverse of every shewy and splendid colour. Woollen yarn dyed crimson or dark blue is the same to me. *Pink* seems to be composed of nine parts of light blue and one of red, or some colour which has no other effect than to make the light blue appear dull and faded a little. Pink and light blue, therefore, compared together, are to be distinguished no otherwise than as a splendid colour from one that has lost a little of its splendour. Besides the pinks, roses, etc., of the gardens, the following British *flora* appear to me blue — namely, Statice Armeria, Trifolium pratense, Lychnis Flos-cuculi, Lychnis dioica, and many of the Gerania. The colour of a florid complexion appears to me that of a dull, opake, blackish blue upon a white ground. A solution of sulphate of iron in the tincture of galls — that is, dilute black ink — upon

white paper gives a colour resembling that of a florid complexion. It has no resemblance of the colour of blood. *Red* and *scarlet* form a genus with me totally different from pink. My idea of red I obtain from *vermilion, minium, sealing-wax, wafers,* a *soldier's uniform,* etc. These seem to have no blue whatever in them. Scarlet has a more splendid appearance than red. Blood appears to me red, but it differs much from the articles mentioned above: it is much more dull, and to me is not unlike that colour called bottle-green. Stockings spotted with blood or with dirt would scarcely be distinguishable.

"*Red* (*by Candlelight*).

"Red and scarlet appear much more vivid than by day. Crimson loses its blue and becomes yellowish red. Pink is by far the most changed—indeed it forms an excellent contrast to what it is by day. No blue now appears: yellow has taken its place. Pink by candlelight seems to be three parts yellow and one red, or a reddish yellow. The blue, however, is less mixed by day than the yellow by night. Red, and particularly scarlet, is a superb colour by candlelight; but by day some reds are the least shewy imaginable—I should call them dark drabs.

"*Orange and Yellow* (*by Daylight and Candlelight*).

"I do not find that I differ materially from other persons in regard to these colours. I have sometimes seen persons hesitate whether a thing was white or yellow by candlelight when to me there was no doubt at all.

"*Green* (*by Daylight*).

"I take my standard idea from grass. This appears to me very different from red. The face of a laurel-leaf (Prunus Lauro-cerasus) is a good match to a stick of red sealing-wax, and the back of the leaf answers to the lighter red of wafers. Hence it will be immediately concluded that I see either red or green, or both, different from other people. The fact is, I believe that they both appear different to me from what they do to others.

Green and orange have much affinity; also apple green is the most pleasing kind to me; and any other that has a tinge of yellow appears to advantage. I can distinguish the different vegetable greens one from another as well as most people, and those which are nearly alike or very unlike to others are so to me. A decoction of Bohea tea, a solution of liver of sulphur, ale, etc., etc., which others call brown, appear to me green. Green woollen cloth, such as is used to cover tables, appears to me a dull, dark, brownish-red colour. A mixture of two parts mud and one red would come near it. It resembles a red soil just turned up by the plough. When this kind of cloth loses its colour, as people say, and turns yellow, then it appears to me a pleasant green. Very light green paper, silk, etc., is white to me."

Then Dalton proceeds to give an account of others whose vision has been similar to his own; and then summarises his case as follows: —

"From a great variety of observations made with many of the above-mentioned persons it does not appear to me that we differ more from one another than persons in general do. We certainly agree in the principal facts which characterise our vision, and which I have attempted to point out below. It is but justice to observe here, that several of the resemblances and comparisons mentioned in the preceding part of this paper were suggested to me by one or other of the parties, and found to accord with my own ideas.

"Characteristic Facts of Our Vision.

"1. In the solar spectrum three colours appear — yellow, blne, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind.

"2. *Pink* appears by daylight to be sky-blue a little faded; by candlelight it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

"3. *Crimson* appears a muddy blue by day, and crimson woollen yarn is much the same as dark blue.

"4. *Red* and *scarlet* have a more vivid and flaming appearance by candlelight than by daylight.

"5. There is not much difference in colour between a stick of red sealing-wax and grass by day.

"6. Dark-green woollen cloth seems a muddy red, much darker than grass, and of a very different colour.

"7. The colour of a florid complexion is dusky blue.

"8. Coats, gowns, etc., appear to us frequently to be badly matched with linings, when others say they are not. On the other hand, we should match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens.

"9. In all points where we differ from other persons, the difference is much less by candlelight than by daylight.

"It appears, therefore, almost beyond a doubt that one of the humours of my eye, and of the eyes of my fellows, is a *coloured* medium, probably some modification of blue. I suppose it must be the vitreous humour, otherwise I apprehend it might be discovered by inspection, which has not been done. It is the province of physiologists to explain in what manner the humours of the eye may be coloured, and to them I shall leave it, and proceed to show that the hypothesis will explain the facts stated in the conclusion of the second part."

The above explanation of his peculiar vision was shown after his death to be erroneous. Mr. Ransome, who made the post-mortem, examined the lenses of Dalton's eyes and found them to be normal to a man of his age. The cause lies much deeper; and the question whether it is due to a defective condition of the retina, or of the optic nerves, or of the brain substance itself, is still a matter of doubt. The subject has been discussed by Herschel, Young, Brewster, and Helmholtz. Young's suggestions on

the theory of colour vision, published in the Philosophical Transactions of 1802, are perhaps those which have been most generally accepted; and his theory is known as the "three-colour theory." According to Young, there are three sets of nerve-fibres in the retina, one of which responds to those vibrations of light which to the normal eye produce the sensation of redness, a second set is sensitive to the green rays, and a third to the violet rays. Thus the first set are excited by the rays of greatest wave-length, the second by those of smaller, and the third by those vibrations of least length of wave. Max Schultze's observations on the minute rods occurring in the retina of birds and reptiles seem to favour this view. For he finds that the anterior extremities of certain of these rods are of a red colour, and are, therefore, capable of absorbing, or are specially sensitive to, the red ray; whilst others end in a yellow drop, and others, again, end in a colourless one. Helmholtz, a great authority, remarks upon these observations that these rods may with great probability be regarded as the terminal organs of the nerve-fibres, which convey respectively the impressions of red, of green, and of blue light. Another theory of colour-vision has been proposed by Hering, who assumes that there are six fundamental colour sensations — viz. white, black, red, yellow, blue, and green. These six sensations may be divided into three pairs, in each of which the one colour is complementary to the other: white to black, red to green,

and yellow to blue. Three sets of nerves in the retina may be able to distinguish these six colours, because it may be imagined that whilst one colour — say, the red — promotes constructive changes (or changes of assimilation) in the nerve matter, the complementary colour, or, in this instance, the green, promotes destructive changes (or changes of dissimilation), and that the chemical changes thus effected in the visual substance may give rise to different sensations.

The difficulties surrounding the subject are, however, great; and both the above theories are inadequate to account for either normal or defective colour-vision. We now know that Newton's and Young's ideas as to the threefold nature of coloured light are incorrect. Instead of there being three primary colours, of which all intermediate tints are constituted, and instead of the solar spectrum being made up of three superimposed spectra — red, yellow, and blue, according to Brewster's view — we now know that each ray of a given degree of refrangibility has a definite and distinct wave-length, and that its colour is not the result of the admixture of two or more superimposed rays of different tints. It would thus seem that our power of discerning colour may be dependent upon the retina being able to discriminate each one of the thousands of different-coloured rays to which the eye is sensitive.

Dalton's publication of these cases of colour-blindness — a name which we owe to Brewster — drew

general attention to the subject, both at home and abroad. Elie Wartmann, of Lausanne, and Prevost, of Geneva, investigated the peculiarity in question, to which they gave the name of "Daltonism"—a designation which fortunately has not come into general use, as many objections apply to the employment of terms of a personal character to describe natural phenomena, although Dalton himself does not seem to have been annoyed, but rather amused, by this use of his patronymic.

The expression "colour-blindness" is also objectionable, because it is not so much blindness as defective vision with which we have to do. George Wilson, whose researches on this question were published in 1855, proposed the name "Chromato-Pseudopsis," or false-colour vision. This is certainly a more correct term, though rather a jaw-breaker. Herschel coined the word "Dichromic," or "two-coloured" vision. But this does not fit the case exactly, because most "colour-blind" eyes can distinguish three colours, and, like Dalton, they see differences between red, yellow, and blue; so that after all the term "colour-blindness" is perhaps the best that has been proposed.

As Dalton has stated, the most serious defect in the eyesight of the colour-blind is in the want of normal perception of red and of its complementary colour, green—one side of a laurel leaf appeared to him "a good match for a stick of sealing-wax," and the other

for a red wafer. He has also pointed out that less confusion exists if the objects are seen by artificial rather than by day light. This observation has been explained by Dove, who concluded that we all become sooner blind to red than to other colours, so that the difference between normal vision and that of a colour-blind person is, after all, only one of degree. This fact is borne out by Wilson's experiments. He examined the power of colour-selection—in the case of skeins of coloured wools or pieces of coloured glass—by colour-blind persons, and found that, to begin with, they chose out the scarlet or red skeins correctly, but did so slowly and somewhat hesitatingly; but that after a while their eyes seemed to become fatigued and they commenced to select wools of a different colour, generally green, and placed these on the red heap, nor was any return to a red skein observed. Whether Dalton's eyes were altogether non-sensitive to the rays of longer wave-length which affect normal sight, as red, is a matter of doubt. He states that the portion of the spectrum which others call red appears to him little more than a shade or defect of light, whilst after that the orange, yellow, and green seem *one* colour, etc. Thus he excludes red, speaking of it as defective, or more or less black. Herschel, writing to Dalton in 1833, takes the view that he and all others so affected perceive as light every ray which those do who possess normal vision.

"The retina," he says, "is excited by every ray which reaches it, nor, so far as I can see, is there the slightest ground for believing that any ray is prevented from reaching it by the media of the eye." This, as we know, was contrary to Dalton's notions; but, as has been explained, Herschel was right so far as Dalton's eye was concerned.

George Wilson was the first to draw public attention to the important fact, now generally acknowledged, that this defect of vision is very much more common than was at one time supposed. He concludes that the proportion of persons in this country whose sight is thus defective amounts to one in twenty. The actual number of male persons of all sorts of occupations and positions, tested by Wilson at Edinburgh, was 1,154, and, out of these, one in fifty-five was markedly colour-blind. The extreme importance of ascertaining the existence of this large proportion of persons unable to distinguish between red and green lights at once became evident. The safety of travellers, both by sea and by land, is jeopardised if the conduct of the train or the steering of the ship is in the hands of guards, pointsmen, or sailors whose eyesight is thus defective; for both at sea and in the railway service red and green lights at night are used — on sea to indicate direction, and on railways to indicate "danger" and "safety"; by day, too, red and green flags are employed for the same purpose.

So well recognised is this fact, that the various railway boards, the military and naval authorities, and other bodies, both at home and abroad, are now in the habit of testing the eyesight of their employees. The tests used by these various authorities are different; some are known to be satisfactory, others the reverse. In consequence of this uncertainty, a committee of scientific experts was lately appointed by the Royal Society to report upon the most reliable method of ascertaining whether vision is normal or abnormal. The necessity for some improved test was made strikingly evident to the committee, inasmuch as when an official of one of the English railways had explained the method adopted by that company for testing the eyesight of their servants, the Assistant-Secretary of the Royal Society, who is colour-blind, was introduced and tested in the manner in use by the company. The official, having made the tests, and having received what he considered to be satisfactory replies to his questions, remarked that Mr. Rix, the Secretary, had good sight, and that he would pass him as normal on an English railway. When informed that Mr. Rix was colour-blind, the official remarked, "I must admit that Mr. Rix being colour-blind is an eye-opener!" It is also an "eye-opener" to the public to be told that any great railway company test the eyesight of their engine-drivers, guards, and signalmen with such precision that some of them are

certainly unable to distinguish between a red "danger" signal and a green "safety" one!

The committee recommended that a central authority, such as the Board of Trade, should schedule certain employments in the mercantile marine and on railways, the filling of which by persons whose vision is defective, either for colour or for form, or who are ignorant of the names of colours, would involve danger to life and property. The proper testing, both for colour and form, of all candidates for such posts must be compulsory. The tests for colour ought to be made by competent examiners, and must consist of the selection by the candidate of skeins of coloured wools, according to the method proposed by a Swedish physicist, Holmgren, now in general use in that country and elsewhere on the Continent.

It further appears, from the evidence given before this committee, that people exist, having normal vision, who are so ignorant, that they are unable to name a red or a green, or even a white, signal when put before them. Such persons are found especially among recruits, who are famous for their stupidity — so much so that they are said sometimes not to know how to put the right leg foremost — and they ought, of course, to be excluded from employments requiring quick and certain observations of colour.

It is a well-known fact that excess of tobacco

smoking produces a defect in vision called by "the profession" "tobacco amblyopia," similar to that of congenital colour-blindness. John Dalton was fond of his pipe—so much so that in his opinion every philosopher ought to smoke; and the only fault that he could find in the character of Sir Humphry Davy was that he abjured the "weed." It is, however, not to be for a moment supposed that Dalton's evening smoke injured his eyesight; on the contrary, it is clear that his was a case of congenital colour-blindness.

[*For further information on this subject see Captain Abney's volume on "Colour Vision." Sampson Low & Co.*, 1895.]

CHAPTER IV.

DALTON'S EARLIER PHYSICAL AND CHEMICAL WORK.

UP to the year 1796 we have no evidence that Dalton had taken any special interest in chemical research, or even had carried on any practical laboratory work. His first introduction to the science, giving him an impetus to its study, seems to have been a course of lectures on chemistry which he attended, given in Manchester by Dr. Garnet. From that time onwards, however, both his mind and his hands were alike constantly occupied in endeavouring to obtain a knowledge of the laws which express the chemical and physical properties of gases. Here it was, he plainly saw, rather than in the case of solids or liquids, that light would come, and to this he bent all the powers of his being. These were sterling honesty of purpose, inflexibility of will, clear-sightedness, and complete devotion to his subject. "If," says he in later life, "I have succeeded better than many who surround me, it has been chiefly — nay, I may say, almost solely — from unwearied assiduity. It is not

so much from any superior genius that one man possesses over another, but more from attention to study, and perseverance in the objects before them, that some men rise to greater eminence than others.' And these words are true enough, although perhaps not expressing the whole truth; for in order to accomplish the greatest things of all something more than mere plodding is wanted. The "Divine Afflatus" must be there, and the scientific imagination must be vivid, if more than a glimpse of Nature's secret ways are to be disclosed. As to how far this power of inspiration was carried in Dalton's case opinions may differ. Some may look upon him only as a slow-witted worker, having but little knowledge or interest beyond the immediate results of his experiments. Others may consider him as one of the great seers of science, dwelling constantly in a realm of thought far beyond the ordinary habitations of mankind, and bringing down for their benefit some of the sweet fruits of a higher world. Probably the truth will be found to lie between these two extremes. All, however, will agree that genius or intellectual insight can accomplish little without perseverance, and that this latter was possessed in high degree by Dalton.

Perhaps the most important of Dalton's physical papers is one read on October 2nd, 16th, and 30th, 1801, on the constitution of mixed gases. This contains a statement of four important laws which form the basis of our present knowledge. The first of these

was arrived at from theoretical considerations, the rest by direct experiment. According to the first, the atmospheric oxygen, being heavier than the nitrogen, ought to diminish as the height increased. This is not found to be the case, and Dalton subsequently (1842) modified his first statement, and expressed himself more in accordance with facts as we now know them. The four laws given by him are:—

"1. When two elastic fluids, denoted by A and B, are mixed together, there is no mutual repulsion amongst their particles; that is, the particles of A do not repel those of B as they do one another. Consequently, the pressure or whole weight upon any one particle arises solely from those of its own kind.

"2. The force of steam from all liquids is the same at equal distances above or below the several temperatures at which they boil in the open air, and that force is the same under any pressure of another elastic fluid as it is *in vacuo*. Thus the force of aqueous vapour of 212° is eqnal to 30 inches of mercury; at 30° below, or 182°, it is of half that force; and at 40° above, or 252°, it is of double the force; so, likewise, the vapour from sulphuric ether, which boils at 102°, then supporting 30 inches of mercury, at 30° below that temperature it has half the force, and at 40° above it, double the force; and so in other liquids. Moreover, the force of aqueous vapour of 60° is nearly equal to $\frac{1}{2}$ inch of mercury when admitted into a Torricellian vacuum; and water of the same temperature, confined with perfectly dry air, increases the elasticity to just the same amount.

"3. The quantity of any liquid evaporated in the open air is directly as the force of steam from such liquid at its temperature, all other circumstances being the same.

"4. All elastic fluids expand the same quantity by heat; and this expansion is very nearly in the same equable way

as that of mercury: at least, from 32° to 212°. It seems probable the expansion of each particle of the same fluid, or its sphere of influence, is directly as the quantity of heat combined with it, and consequently the expansion of the fluid as the cube of the temperature reckoned from the point of total privation."

Dalton's apparatus was, as has been said, always of the simplest, and often of the roughest, kind. It may be interesting to see how he determined the tension of aqueous vapour, and ascertained that the vapours of all liquids at equal distances from their respective boiling-points have the same tension.

"My method is this: I take a barometer-tube perfectly dry, and fill it with mercury just boiled, marking the place where it is stationary: then having graduated the tube into inches and tenths by means of a file, I pour a little water (or any other liquid the subject of experiment) into it, so as to moisten the whole inside; after this I again pour in mercury, and, carefully inverting the tube, exclude all air. The barometer by standing some time exhibits a portion of water, etc., of $\frac{1}{2}$ or $\frac{1}{10}$ of an inch upon the top of the mercurial column: because being lighter it ascends by the side of the tube, which may now be inclined, and the mercury will rise to the top, manifesting a perfect vacuum from air. I next take a cylindrical glass-tube open at both ends, and of 2 inches diameter and 14 inches in length, to each end of which a cork is adapted, perforated in the middle so as to admit the barometer-tube to be pushed through and to be held fast by them; the upper cork is fixed two or three inches below the top of the tube, and is $\frac{1}{2}$ cut away so as to admit water, etc., to pass by, its service being merely to keep the tube steady. Things being thus circumstanced, water of any temperature may be poured into the wide tube, and thus made to surround the upper part or vacuum of the barometer, and the effect of temperature in the production of vapour within can be

observed from the depression of the mercurial column. In this way I have had water as high as 155° surrounding the vacuum; but as the higher temperatures might endanger a glass apparatus, instead of it I used the following:—

"Having procured a tin tube of 4 inches in diameter and a foot long, with a circular plate of the same soldered to one end, having a round hole in the centre like the tube of a reflecting telescope, I got another smaller tube of the same length soldered into the larger, so as to be in the axis or centre of it; the small tube was open at both ends, and on this construction water could be poured into the large vessel to fill it, whilst the central tube was exposed to its temperature. Into this central tube I could insert the upper half of a syphon barometer, and fix it by a cork, the top of the narrow tube also being corked; thus the effect of any temperature under 212° could be ascertained, the depression of the mercurial column being known by the ascent in the exterior leg of the syphon. The force of vapour from water between 80° and 212° may also be determined by means of an air-pump, and the results exactly agree with those determined as above. Take a Florence flask half-filled with hot water, into which insert the bulb of a thermometer, then cover the whole with a receiver on one of the pump plates and place a barometer gage on the other; the air being slowly exhausted, mark both the thermometer and barometer at the moment ebullition commences, and the height of the barometer gage will denote the force of vapour from water of the observed temperature. This method may also be used for other liquids. It may be proper to observe the various thermometers used in these experiments were duly adjusted to a good standard one.

"After repeated experiments by all these methods and a careful comparison of the results, I was enabled to digest the following table—the force of steam from water in all the temperatures from 32° to 212°."

In the fourth essay of this series Dalton discusses another most important physical property of gases

and vapours, or, as he terms them, "elastic fluids" —viz., their expansion by heat. He first quotes the discordant results obtained by Berthollet and Monge, according to which the expansion of one gas differed widely from that of another. "These conclusions were so extremely discordant with and even contradictory to those of others that I could not but suspect some great fallacy in them, and found it in reality to be the fact. I have no doubt it arose from the want of due care to keep the apparatus and materials free from moisture." Dalton next proceeds to describe the methods he adopted in order to exclude this and other sources of error from his experiments, and then gives his conclusions in the following remarkable words:—

"The results of several experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas, and nitrous gas—which were all the kinds I tried—agreed with those on common air, not only in the total expansion, but in the gradual diminution of it in ascending. . . . Upon the whole, therefore, I see no sufficient reason why we may not conclude that all elastic fluids under the same pressure expand equally by heat, and that for any given expansion of mercury the corresponding expansion of air is proportionally something less the higher the temperature. This remarkable fact that all elastic fluids expand the same quantity in the same circumstances plainly shews that the expansion depends solely upon heat, whereas the expansion in solid and liquid bodies seems to depend upon an adjustment of the two opposite forces of heat and chemical affinity: the one a constant force in the same temperature, the other a variable one according to the nature of the body—hence the unequal

expansion of such bodies. It seems, therefore, that general laws respecting the absolute quantity and nature of heat are more likely to be derived from elastic fluids than from other substances."

This law of equal expansion of all gases for equal increments of temperature has been generally known on the Continent as "Gay-Lussac's" or "Charles's law," but ought to be called "Dalton's law of expansion," as he first announced it and gave experimental evidence of its truth, and the claims of the Manchester philosopher are generally now allowed. In 1804, however, these questions were under discussion, and he writes: "My lately published essays on gases, etc., together with the more recent ones read at our society, and of which I gave the result in my late lectures, have drawn the attention of most of the philosophers of Europe. They are busy with them at London, Edinburgh, Paris, and in various parts of Germany, some maintaining one side and some another. The truth will surely out at last."

In following the progress which Dalton's mind makes towards the most fruitful of his ideas — that of the Atomic Theory of chemistry — it is interesting to note that at the end of this essay he gives a diagram to illustrate his conception of the constitution of the atmosphere. He gives to the particles of the different gases contained in the air separate signs, and so arranges these signs or marks — the space for each gas being equal — that the marks indicate the real

densities of the gases in question. This shows that his mind was occupied with the atomic conception that gases are composed of definite particles. Thus he states "that homogeneous elastic fluids are constituted of particles that repel one another with a force decreasing as the distance of their centres from each other." "There is, therefore, not any doubt," as Dr. Henry says, "that it was in contemplating the essential condition of elastic fluidity that he first distinctly pictured to himself the existence of atoms."

The next important research is an "Experimental Enquiry into the Proportion of the Several Gases or Elastic Fluids constituting the Atmosphere," read November 12th, 1802. This is interesting, in the first place as giving the result of his analyses of the air in Manchester near the sea's level and also at the summit of Helvellyn — the results showing no perceptible difference in the composition of the air taken from these two sources — a difference which, according to his views as already stated, ought to exist owing to the difference in specific gravity of oxygen and nitrogen, although this expected variation could scarcely be observed by his methods of analysis.

In spite of his rough methods, Dalton here, as elsewhere, obtained results the correctness of which has been borne out by later work. Especially is this the case with the question of the constant composition

of the atmosphere as regards oxygen and nitrogen. Thus he finds that "the bulk of any given quantity of atmospheric air appears to be reduced nearly 21 per cent. by abstracting its oxygen" — a statement, be it observed, which holds good to-day. Then he continues: "This fact, indeed, has not been generally admitted till lately" (doubtless referring to Cavendish's previous determinations, in which it was shown that the air of London — Marlborough Street — and the country — Kensington — had the same composition, containing on an average 20·83 per cent. of oxygen), "some chemists having found, as they apprehended, a great difference in the quantity of oxygen in the air at different times and places; on some occasions 20 per cent., and on others 30 and more, of oxygen are said to have been found." "This, I have no doubt," Dalton wisely and truly adds, "was owing to their not understanding the nature of the operation and of the circumstances influencing it. Indeed, it is difficult to see, on any hypothesis, how a disproportion of these two elements should ever subsist in the atmosphere."

Subsequent more exact determinations have shown that certain small variations do, in fact, exist, but that any permanent diminution in the percentage of oxygen at high elevations is not traceable, doubtless owing to the atmospheric disturbances, and also to the fact — also discovered by Dalton — that gases act as vacua to one another.

Another and still more interesting result was arrived at by Dalton in the course of these experiments, and shows the direction in which his ideas were moving. In his air-analyses he used the well-known method of mixing the air with nitrous gas (our nitric oxide) when the oxygen is absorbed with formation of soluble red fumes due to the union of the atmospheric oxygen with nitrous gas. This process, he remarks, has been much discredited by late authors; but he considers it to be not only the most elegant and expeditious of all, but also as correct as any of the others. Now comes the interesting observation—the first germ of his great discovery of the law of chemical combination in multiple proportions. He finds that if 100 measures of common air be put to 36 of pure nitrous gas in a tube $\frac{3}{10}$ of an inch wide and 5 inches long, after a few minutes the whole will be reduced to 79 or 80 measures and exhibit no signs of either oxygenous or nitrous gas. If, on the other hand, 100 measures of common air be admitted to 72 of nitrous gas in a wide vessel over water, such as to form a thin stratum of air, and an immediate momentary agitation be used, then there will, as before, be found 79 or 80 measures of pure azotic gas (nitrogen) for a residuum. But if in the last experiment *less* than 72 measures of nitrous gas be used, there will be a residuum containing oxygenous gas; if more, then some residuary nitrous gas will be found. Now for Dalton's explanation,

which, as I have said, is the first inkling of his great law according to which chemical combination proceeds.

"These facts," says Dalton, "clearly point out the theory of the process. The elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former case *nitric* acid is the result, in the latter *nitrous* acid; but as both these may be formed at the same time, one part of the oxygen going to *one* of nitrous gas, and another to *two*, the quantity of nitrous gas absorbed should be variable—from 36 to 72 per cent. for common air. This is the principal cause of that diversity which has so much appeared in the results of chemists on this subject. In fact . . . the wider the tube or vessel the mixture is made in, the quicker the combination is effected; and the more exposed to water, the greater is the quantity of *nitrous* acid and the less of *nitric* that is formed." *

Soon after the publication of the last paper Dalton read one upon the Tendency of Elastic Fluids to Diffusion. Everybody knows that oil and water do not mix, so also if spirits of wine is gently poured on to the top of water the film of lighter spirit remains floating on the water, although if the two

* This was written before November, 1802, but not printed till November, 1805. The first edition of the "New System of Chemical Philosophy" is dated May, 1808.

be stirred perfect mixture occurs, Priestley having already noticed that when gases which do not act chemically upon one another, but are of different specific gravities, like spirits and water, are once mixed they do not again separate; but he thought that just as spirits and water, when carefully brought together, remain distinct, so two gases of different densities might also remain in layers one above the other. This question was the one which Dalton wished to answer; or, as he puts it, Can a lighter elastic fluid (gas) rest upon a heavier one? and he proved by a very simple experiment that it cannot do so.

The only apparatus found necessary was a few phials and tubes with perforated corks; the tube mostly used was one 10 inches long and of $\frac{1}{20}$ inch bore; in some cases a tube 30 inches in length and $\frac{1}{3}$ inch bore was used; the phials held the gases that were the subject of the experiment, and the tube formed the connection. Dalton substituted in many experiments a long "churchwarden" clay pipe for the glass tube. "In all cases, the heavier gas was in the *under* phial, and the two were placed in a perpendicular position, and suffered to remain so during the experiment in a state of rest; thus circumstanced, it is evident that the effect of agitation was sufficiently guarded against; for a tube almost capillary and 10 inches long could not be instrumental in propagating an admixture

from a momentary commotion at the commencement of each experiment." The gases experimented on were atmospheric air, oxygen, hydrogen, nitrogen, nitrous oxide, and carbonic acid. After the gases had remained in contact as described for a given length of time, the composition of the gas in each phial was determined, and the analysis invariably showed that a passage of the heavier gas upwards and the lighter gas downwards had occurred; or, in other words, gases diffuse mutually into each other.

We had to wait for a knowledge of the laws of gaseous diffusion for upwards of thirty years, for it was not until 1834 that Graham discovered that the rate of diffusion of a gas is inversely proportional to the square root of its density. Thus a given volume of oxygen, which is sixteen times as heavy as hydrogen, takes four times as long to diffuse as the same volume of hydrogen. Many years had again to elapse before the true significance of Graham's law was understood. In 1848 Joule proved the correctness of Bernouilli's views that the pressure of the air could be explained by the impact of its particles on the walls of the containing vessel, and he calculated the mean velocity which the molecules of the gas — these being constantly in motion — must possess in order to effect the observed pressure.

Dalton's words, which follow, come very close to the modern explanation: —

"1. The diffusion of gases through each other is effected by means of the repulsion belonging to the homogeneous particles, or to that principle which is always energetic to produce the dilatation of the gas.

"2. When any two or more mixed gases acquire an equilibrium, the elastic energy of each against the surface of the vessel, or of any liquid, is precisely the same as if it were the only gas present occupying the whole space, and all the rest were withdrawn."

Clausius, Maxwell, and other physicists, have extended and completed the dynamical theory of gases, and applying to these molecules the well-known laws of masses in motion, the laws of gases, as found by experiment — viz. Boyle's law of pressures, Dalton's law of expansion, Graham's law of diffusion, and what is usually known as "Avogadro's theory" (to which reference will again be made) — all find their theoretical explanation, and have for their existence a sound physical foundation.

The phenomena of the solubility of gases in water next attracted Dalton's attention, especially as regards the solubility of the gases under variation of pressure. We all know that when water is saturated with gas under pressure, as in soda-water, and when the pressure is removed, the gas escapes with effervescence. The question as to the relation between amount of gas and pressure had been made the subject of

experiment. Dalton's old friend, Dr. William Henry, had already communicated to the Royal Society the results of his experiments on the same subject, expressing what is now known as "Henry's law" — viz. that the amount of gas absorbed by water varies directly as the pressure.

Dalton asked himself, What will happen if a mixture of two gases, such as oxygen and nitrogen, is shaken up with water? On making the experiment he found that the quantity of each gas absorbed is independent of the other — that is, supposing the mixture to be one-half oxygen and one-half nitrogen, the amount of oxygen dissolved is exactly half what it would have been if the space occupied by the nitrogen had been a vacuum; and in the same way exactly half as much nitrogen is absorbed as would have been the case had the space occupied by the oxygen been vacuous. This is known as "Dalton's Law of Partial Pressures," and both Henry's and Dalton's laws have stood the test of time.

Dalton's views as to how the gas is dissolved in the water are interesting, because they shadow forth his coming Atomic Theory. His mind was, as has been said, of a corpuscular turn. Having this habit of mind, he conceived the particles of gas amongst those of water; "the former retained their elasticity or repulsive power amongst their own particles, just the same in the water as out of it, the water having no other influence in this respect than a

mere vacuum, and no other gas with which the first one is mixed having any permanent influence in this respect." He believed that the different gases which do not combine chemically with the water are absorbed in certain definite fractions of the bulk of the water, in amount corresponding to the cubes of the reciprocals of the natural numbers—1, 2, 3, etc., or $\frac{1}{1^3}, \frac{1}{2^3}, \frac{1}{3^3}, \frac{1}{4^3},$ and so on—the same gas always being in the same proportion, provided the temperature and pressure remain constant. He looked upon the phenomenon as a purely mechanical one, the particles of the gas taking a certain position amongst those of the water; just as we may imagine grains of sand strewed regularly amongst a quantity of small shot. Dalton gives pictures of this arrangement—"a horizontal view of air in water" and "a profile view of air in water"—with marks where the particles of the different gases are supposed to lie. It is scarcely necessary to remark that this mere mechanical view of the matter will not hold water. We now know that the question has to be looked at in a totally different light, that Dalton's idea of definite fractions is altogether erroneous, and that every gas possesses a fixed co-efficient of solubility, which is constant for every given temperature, and which cannot be ascertained by calculation, but must be determined by experiment. In other words, we do not yet know the law regulating solubility and temperature, nor

can we explain why one gas is more soluble than another.

But although Dalton's theory of solubility was altogether mistaken, it was one of the most fruitful theories ever devised, because it led him to the greatest of all his discoveries—namely, to the determination "of the *relative weights* of the ultimate particles of gaseous and other bodies." How exactly this came about, and why the numbers which he puts down in this first published table of atomic weights were pushed in, as it were, neck and crop at the end of this essay on the solubility of gases, will always remain a mystery. It must suffice us that there it stands on page 287 of the first volume of the new series of the "Memoirs of the Literary and Philosophical Society of Manchester"—having been read on October 21st, 1803, but published in November, 1805—as the great foundation-stone in chemical science; for upon it and what naturally followed it all the edifice of modern chemistry rests. This is the form in which it first saw light:—

TABLE

OF THE RELATIVE WEIGHTS OF ULTIMATE PARTICLES OF GASEOUS AND OTHER BODIES.

Hydrogen	1	Phosphorus	7·2
Azot	4·2	Phosphuretted hydrogen	8·2
Carbone	4·3	Nitrous gas	9·3
Ammonia	5·2	Ether	9·6
Oxygen	5·5	Gaseous oxide of carbone	9·8
Water	6·5	Nitrous oxide	13·7

TABLE OF THE RELATIVE WEIGHTS OF ULTIMATE PARTICLES OF GASEOUS AND OTHER BODIES — (*continued*).

Sulphur	14·4	Sulphureous acid ...	19·9
Nitric acid	15·2	Sulphuric acid	25·4
Sulphuretted hydrogen	15·4	Carburetted hydrogen from stag. water ...	6·3
Carbonic acid	15·3	Olefiant gas	5·3
Alcohol	15·1		

The only explanation which Dalton vouchsafed is given in the following words: —

"The greatest difficulty attending the mechanical hypothesis arises from different gases observing different laws. Why does water not admit its bulk of every kind of gas alike? This question I have duly considered, and, though I am not able to satisfy myself completely, I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases — those whose particles are lightest and single being least absorbable, and the others more, according as they increase in weight and complexity.* An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new; I have lately been prosecuting this inquiry with remarkable success. The principle cannot be entered upon in this paper; but I shall just subjoin the results as far as they appear to be ascertained by my experiments."

It is, therefore, clear that it was by experiment and not by mere reasoning that Dalton obtained the

* "Subsequent experience renders this conjecture less probable."

above numbers. What the exact nature of these experiments was it is not easy to infer, as he does not explain how he arrived at them. They were probably in part his own, but also, and to a considerable extent, those of other chemists, whose results Dalton made use of in calculating his numbers. We shall have to return to this subject when discussing the Atomic Theory.

CHAPTER V.

DALTON'S DAILY LIFE.

DALTON'S daily wants were of the simplest, and his habits most uniform. When he left the College he lived for a time in a house in Faulkner Street, and worked in rooms placed at his disposal as laboratory and study in the house of the Literary and Philosophical Society, then, as now, situated at 36, George Street, Manchester. Not long afterwards he removed to the house of John Cockbain, a member of the Society of Friends. After residing with him for some time, Dalton in 1805 went to live in the house of a friend, the Rev. William Johns, in Faulkner Street—a street long since entirely given up to warehouses and offices—hard by, and there he continued to reside until the year 1830, when Mr. Johns left, and Dalton took a house and lived alone.

How this residence with the Johns family came about is thus related by Miss Johns, the daughter. "As my mother was standing at her parlour window, one evening towards dusk, she saw Dr. Dalton passing on the other side of the street, and, on her opening the window, he crossed over and greeted her. 'Mr. Dalton,' said she, 'how is it that you so seldom come

to see us?' 'Why, I don't know,' said he; 'but I have a mind to come and live with you.' My mother thought at first that he was in jest; but finding that he really meant what he said, she asked him to call again the next day, after she should have consulted my father. Accordingly he came and took possession of the only bedroom at liberty, which he continued to occupy for nearly thirty years. And here I may mention, to the honour of both, that throughout that long connection he and my father never on any occasion exchanged one angry word, and never ceased to feel for each other those sentiments of friendly interest which, on the decline into years of both, ripened into still warmer feelings of respect and affection."

The following graphic description of his life in Manchester was given by his lifelong friend, Miss Johns, who had ample opportunity of becoming acquainted with Dalton's moral qualities and domestic tastes: —

"The doctor's habits of life," she writes, "were so uniform and unvaried as to be soon related. On Sundays he always dressed himself with the most scrupulous attention to neatness, attended public worship twice — except when indisposed, or on very particular occasions, which, however, the writer does not remember to have occurred a dozen times in all — dined, during his life, with his friend Mr. Thomas Hoyle, the printer of Mayfield, and, returning home to tea, spent the evening in his philosophical pursuits. His dress was that usually worn by the Quakers, avoiding, however, the extreme of formality and always of the finest texture; hat, gloves, gaiters, and even a handsome cane to correspond. In his general intercourse,

also, he never adopted their peculiar phraseology. With respect to his religious principles, I should be disposed to think that he had never made theology, properly so called, a study. He certainly never mentioned having done so; but his reverence for the great Author of all things was deep and sincere, as also for the Scriptures, in which His revealed will is expressed. When the occasion called for it, I have heard him express his sense of the duty and propriety of the religious observance of Sunday, and also his serious disapprobation of its violation. Although frequently solicited, he refused all invitations to dine out on that day, except a very few times at Dr. Henry's, and once or twice elsewhere, when, as he observed to me, he was asked to meet a very distinguished professor whom he should otherwise have missed the opportunity of seeing. But when the same friend, presuming on his former compliance, again invited him on that day he received a refusal, which prevented any further application. His week-days — every day, and all day long — were spent in his laboratory, with the exception of Thursday afternoons, when he accompanied a party of friends about three miles into the country to bowl, and entered into the amusement with a zest infinitely amusing to all who were present. He also spent a few minutes, generally between light and dark, at the Portico, in reading the daily papers. He rose about eight in the morning, always lighted his laboratory fire before breakfast, after which meal he finished his toilet and repaired to his laboratory, which he seldom left until dinner. He dined at one, but always came in in much haste when dinner was partly over — I suppose to save time. He ate moderately, and drank only water. He was obliged to eat slowly on account of the conformation of his throat, which was very narrow. After dinner he always spent about a quarter, rarely half an hour, in chatting with the different members of the family, or any visitor, or in looking over any chance publication lying on the table. After spending the afternoon in his laboratory, he drank tea at five, rarely coming in until the family had nearly finished. He was very methodical in the quantity he took at meals. After tea,

to his laboratory again, where he staid until nine (supper-time), when he regularly shut up for the night, ate a light supper, generally of meat and potatoes, until about his sixtieth year, when he changed this for meal porridge, with milk or treacle, or occasionally a couple of eggs. After supper we all sat together, and generally had a nice chat, for which the labours of the day had excellently prepared us all; and I will venture to say that few fire-sides have ever presented a scene of more innocent and pleasant recreation than did ours during these the busy years of our life. The doctor took little part in the conversation, though he shewed that he listened by frequently smiling, and now and then uttering some dry, laconic witticism in reference to what was passing. He and my father smoked their pipes unremittingly. Not unfrequently we were joined by two or three political friends, who talked over the news of the times, etc. The doctor enjoyed their society, but took little part in the conversation, in politics none whatever, nor for years had we any idea what his views on the subject were (Conservative). Occasionally he took the chief part in conversation; but this only when we were quite alone, or when Mr. Ewart stepped in, as he sometimes did. He and the doctor had a great esteem for each other, which lasted through life. When, however, this gentleman was our visitor, the evening seldom ended without my father and he getting deeply into metaphysics — a favourite study with both. The doctor generally listened intently, but, from an occasional ironical smile, I used to suspect that he thought it mostly 'vain wisdom all and false philosophy.' My sister Catherine's wit and animated nonsense were, I fancy, more to his taste. When we had no company we always withdrew before eleven, when the doctor pursued his meditations for nearly an hour longer, and then, having perambulated the lower part of the house to see that all the fires were out, he himself went to bed, and by midnight all were at rest."

That he was able not only to make 200,000 meteorological observations, but to accomplish so much

more, depended to a great extent on his methodical habits. His life went as by clockwork. A lady, who lived nearly opposite his laboratory in George Street, used to say that she knew the time to a minute by seeing Dr. Dalton open his window to read off the height of his thermometers. His observance of method and his punctuality were noticeable in all his doings. For many years President of the Literary and Philosophical Society, he was punctilious as to the commencement of the business of the meeting at seven, and even more so as to its termination at nine. One of his intimates was Peter Clare, one of the secretaries of the Society, and a clockmaker by trade. At Dalton's request, Peter presented to the Society one of his best clocks (it now stands in the meeting-room), which only sounds one stroke on the bell during the twenty-four hours, and that stroke is at nine o'clock in the evening; thus notice was given that the meeting must conclude.

Not only in his work, but in his recreations, the same method prevailed. His summer holidays were usually spent at the English Lakes, where, as we shall see, he occupied himself always in the same way. When living in Manchester his only relaxation from the round of daily work was on Thursday afternoon, which he regularly devoted to a game of bowls on the green of the "Dog and Partridge"—then in the country, but now closed in by grimy streets. He there appears to have enjoyed himself with some

chosen spirits, watching the bowls as they roll with the anxiety of the confirmed player, and showing, as Angus Smith remarks, a glimmer of the latent enthusiasm of his mind by swaying his body, indicating the course which he wished his bowl to take, as if endeavouring to influence it. Methodically, he played only a certain fixed number of games, got his tea at the inn, smoked his "churchwarden" in the company of his fellow-players, and went "home" to his laboratory — for this was, in truth, his home far more than his rooms in the neighbouring house, where he consumed the victuals needed to supply the metabolism of the body, and where he slept in order to obtain further nerve-power for next day's brain-work. He used to say that he went to the "Dog and Partridge" on Thursdays because he preferred his Saturday half-holiday in the middle of the week.

He took no part, and probably little interest, in the political or even in the social questions of the hour. It is true that he went every day for a few minutes to the Portico Library near his laboratory to read the papers, but not even to his intimate associates — he can scarcely be said to have had intimate friends — did he speak either on religion or on politics; indeed, the Johns family did not know that he was a Conservative, and some thought that he was a Liberal; so we may put him down as Liberal-Conservative, which perhaps may be defined as a man who thinks

things ought to progress, but would rather they remained as they are.

In a paper read in 1821 on "The Dew-Point, etc., from Observations made in the North of England," Dalton says: "As I had for some years been in the habit of allowing myself a week or two in summer for relaxation from professional engagements, and had generally spent the time in the salubrious air of the mountains and lakes near my native place in the North of England, it was, therefore, an additional gratification to be enabled to unite instruction with amusement. I began my observations in 1803, and have continued them almost in every successive year to the present."

Helvellyn was the mountain he most frequently ascended, having been on its summit forty times. "I have had a portable barometer not less than seven times upon the summit, and can fully answer for the accuracy of the barometrical variation between the Valley of Wythburn at the foot and the summit of the mountain . . . . from barometrical results I calculate the height of the road at the foot of the mountain to be 180 yards above the level of the sea; also the summit of the mountain to be 850 or 860 yards above the road, making a total of 1,035 yards perpendicular elevation above the sea." The Ordnance Survey makes Helvellyn 1,039 yards.

His summers were spent, as has been said, at the English Lakes. He did not usually make these

excursions alone. Often he met there old Cumberland friends, or he was accompanied by some of the young people of the Johns family. These walks were not only of value from the point of view of meteorology, but also from that of health, for Dalton says, "They were undertaken partly with the object of bringing into exercise a set of muscles which otherwise would have grown stiff." "To those who have seen him only on ordinary occasions," Miss John writes, "it is impossible to convey an idea of his enthusiasm on those occasions. He never wearied."

Jonathan Otley, the well-known writer of the first good guide to the Lake Mountains, who often went with Dalton on these excursions, thus describes some of his wanderings with the great Manchester chemist: —

"On the 6th day of July, 1812, I first met with Mr. Dalton on Skiddaw. Observing that he carried a barometer, I introduced myself by saying that I had seen a little of the use of the barometer in measuring the heights of mountains, having about three years previously accompanied the Rev. Dr. Pearson to Skiddaw, where I saw its application; and he having left the instrument in my possession, I had afterwards made some experiments with it myself. Mr. Dalton then told me he had twice, with a guide from Langdale, attempted to reach the top of Scawfell, but the weather at both times becoming unfavourable, the result had not been satisfactory; and that he proposed to reach it from Wasdale Head, that he expected two friends from Kendal to join him next day, and if I would go along with them they would be glad of my company. Accordingly at noon next day his two friends arrived, and each of us being formally introduced by name — John Dalton, Thomas Wilson, Wilson Sutton, and Jonathan Otley — off we set for Wasdale Head, where

we expected to obtain quarters for the night at some of the farmhouses.

"Passing through Borrowdale, we called at the black-lead mine, where we saw a good quantity of the mineral, as they had recently opened out a productive sop. From thence, shaping our course over the trackless mountain towards Wasdale Head, the clouds lowered so fast that we were presently enveloped in a thick mist. And in our anxiety to reach onr destination before the early time at which the inhabitants of these vales retire to rest Mr. Dalton was leading the way at a brisk pace, while the other two gentlemen were not able to keep up. I, in the middle place, had some difficulty in keeping them all within sight, and in snch a case it would not have been safe to have been separated farther. Having passed the first ridge and descended a little towards the lower edge of the mist, the first object that came in view was a huge rock, which, from its indistinct appearance, I at first took to be Lingmel Crag, on the further side of Wasdale Head; but on getting a little lower we were undeceived by seeing the bright silvery stream of the Lisa meandering down the Vale of Ennerdale on its way to the lake. Having reached the peaceful vale of Wasdale Head — a place at that time not much visited by strangers — we began to look out for lodgings. At the first house, Mr. Isaac Fletcher's, we obtained lodgings for two, which was very acceptable to our two Kendal friends; Mr. Dalton and I journeying on to Mr. Thomas Tyson's, where we found good accommodation.

"Dr. Dalton usually travelled by stage as far as the coach served his purpose; the rest of his journeyings were chiefly accomplished on foot. He used to say that a little mountain exercise brought into play a certain set of mnscles which would otherwise turn rigid and inactive. He was active and persevering in climbing a mountain; especially when he came in sight of the goal there was no keeping pace with him. In descending, or on rough ground, I was fully his equal: my stronger shoes enabled me to venture more freely. The barometer which he carried was of the most simple construction, yet its

action was more intelligible than some fitted up in a more expensive way. His eyes, though subject to some defects, were very exact in estimating small divisions of space. His mode of calculating altitudes generally came out something higher than what has subsequently been given in the Ordnance Survey; but for his purpose the greatest exactness was not required. In later years he declined bringing his barometer, as he had the privilege of using one belonging to the Rev. Dr. Pearson, and afterwards one of my own construction. He was never averse to taking Matthew Jopson's advice in taking a little brandy to mix with the water of Brownrigg Well, but he was very abstemious in using it. Although these excursions have been undertaken chiefly as recreations, they have not been without their use. They assisted in the investigation of the constitution of the atmosphere, and we have been enabled to make a step in advance of our predecessors in the geographical delineation of the district. Although the doctor always treated me as a companion, he would never permit me to go without some pecuniary remuneration—I must not say for loss of time, as no time could be said to be lost that was spent in his company, he was so affable and communicative. When, on the last-mentioned occasion, I would have declined what he offered, he said I must take it; it might probably be the last—and, as far as regarded mountain excursions or journeying in company, so it was. I saw him at Keswick two or three times after that, but still with a kind of melancholy pleasure."

Dalton never forgot his native village and its bucolic but original inhabitants, and he often revisited the home of his childhood. He used to lodge at the Globe Inn, Cockermouth, where he entertained some of his friends to supper. Next morning he rose betimes and made his way to Eaglesfield, where he was always glad to meet his old "weel-kennt" acquaintances. He spoke his native Cumbrian as

well in his old age as when as a youngster he ploughed his father's fields. "What, ye'll be thrang w'yer hay," he would remark to the yeomen as they tossed the sweet-smelling grass of the uplands; and being asked indoors, he would sit down and light his pipe and "have a real gude crack" about old days. There he would meet his former friend and associate, William Alderson, who, sitting over the fire in his cottage, and not seeing any use in a fender, would kick the stray cinders behind the grate with the wooden clogs which all the North-country people then wore.

Often "the Doctor's" party at the Lakes was a large one, and night-quarters had to be sought in some of the farmhouses in the outlying districts. In those days there were none of the great hotels in which travellers by the hundred can now be accommodated, and many shifts had to be resorted to by landlords and their families to put up a belated guest. Once, describing to a young lady friend his adventures in Lake-land, Dalton was asked whether he had ever seen the celebrated beauty of that day, Mary of Buttermere, the daughter of the landlord of the only inn, "The Fish," in that district. "No," said the Doctor, "but I have slept in her bed; for one night I arrived at Buttermere, wet and tired, to find the inn full; but by dint of persuasion a room was found for me, and Mary got out of her bed and I got in, and right warm I found it, I can tell thee."

In spite of his "infirmity of vision," Dalton was

alive to, and impressed by, the beauties of Nature, as the following extract from his journal in 1796 shows: —

"August 22nd, 1796.

"We had a pleasant ride from Kendal for eight miles, when the grand scenery of the Lakes opened upon us with full force. The head of Windermere and about half of the lake, with the surrounding hills skirted with wood, formed a fine and capacious amphitheatre which we had in view more or less till we arrived at Lowood. Drank tea there, and immediately after took a boat out to a central part of the lake, when we beheld the sun descending below the summit of Langdale Pikes, whilst its rays still continued to gild the delightful landscape on the opposite shore. . . . Came off the lake, then proceeded to Ambleside, winding round the still lake by twilight. Went out about ten to view the night scene. The atmosphere was as clear as possible: Jupiter and the fixed stars shone with uncommon splendour and suggested an unusual proximity. The moon, risen, but not above the mountains, cast a glimmering light upon the rocky hills just opposite and produced a fine effect. These circumstances, together with the awful silence around, would have persuaded us we had been transferred to some other planet."

Dalton's first impressions of London, which he visited in May, 1792, are worth recording, and they were not favourable. "London," he says, "is a most surprising place, worth one's while to see once; but the most disagreeable place on earth for one of a contemplative turn to reside in." He does not appear, on this occasion, to have made the acquaintance of any of the Metropolitan *savants* with whom he afterwards became intimate. The object of his visit was a

religious one, and arose from a wish to attend the yearly meeting of "Friends," who, in the spring month, are in the habit of congregating in London from all parts of the kingdom. If in science he was an innovator, so, too, in the religious services of the Quaker meeting-house he appears to have desired to introduce new methods, for he and another equally ardent spirit actually proposed to the assembled elders that organ-music should be used in the "silent service." But to praise God by machinery was, and is still, thought to savour of too much of the stage, and the proposal, as we might imagine, fell very flat.

Dalton often used to say that he had no time to marry; but for all that, in his more youthful days at any rate, he was not insensible to the charms of the fair sex. On looking through the Hortus Siccus made by Dalton, and now in the possession of the Literary and Philosophical Society of Manchester, I came upon a dried specimen of the lady's slipper (*Cypridium calceolus*) — a charming orchid, very abundant in the Yorkshire dales in 1790, but now scarce even in its most favourite haunts. Under this specimen I found in Dalton's handwriting the following inscription: "Presented to me by Nancy Wilson, of Thornton-in-Craven." That Nancy had touched the staid Quaker to his heart's core appears from the following remarks of Miss Johns, who knew him better than almost anyone else: "He used to mention with the warmest interest, and with deep sensibility,

a most amiable and accomplished Quaker friend who died young, but whose memory he ever cherished with the fondest regret. There was nothing but friendship in this, as she was engaged when he became acquainted with her. He had a letter and some verses of this lady's, with which we could by no means prevail upon him to part, or even to let us look at, though he read them to us with a faltering voice, and, what was very rare with him, with eyes suffused with tears, repeating as he ended, 'Poor Nancy, poor Nancy.'"

In a letter to Robinson he allows that he had fallen a victim to the blandishments of the handsomest woman in Manchester, and a widow to boot. But this, it seems, was a transient affair. Probably more serious for the young man than the case of the widow was his acquaintance with Hannah, the daughter of a Lancaster Friend. The following letters describe his feelings so well that they are worth reproducing. Observe his caution to his brother Jonathan: "I would not have thee communicate my sentiments to others."

"It seems that another of your maids is become mistress—a good omen for the next, whoever she may be. Methinks there may be a question started from some side of the fire when this is read—'I wonder whether John is going to marry yet or not?' I may answer that my head is too full of triangles, chymical processes, and electrical experiments, etc., to think much of marriage. I must not,

however, omit to mention that I was completely Sir Roger de Coverleyed a few weeks ago.

"The occasion was this: Being desired to call upon a widow — a Friend, who thought of entering her son at the academy — I went, and was struck with the sight of the most perfect figure that ever human eyes beheld, in a plain but neat dress; her person, her features, were engaging beyond all description. Upon inquiry after, I found she was universally allowed to be the handsomest woman in Manchester. Being invited by her to tea a few days after, along with a worthy man here, a public Friend (a Quaker minister), I should have in any other circumstances been highly pleased with an elegant tea equipage, American apples of the most delicious flavour, and choice wines: but in the present these were only *secondary* objects. Deeming myself, however, full proof against *mere beauty,* and knowing that its concomitants are often ignorance and vanity, I was not under much apprehension. But she began to descant upon the excellence of an exact acquaintance with English grammar and the art of letter-writing; to compare the merits of Johnson's and Sheridan's dictionaries; to converse upon the use of dephlogisticated marine acid in bleaching; upon the effects of opium on the animal system, etc. etc. I was no longer able to hold out, but surrendered at discretion. During my *captivity,* which lasted about a week, I lost my appetite, and had other symptoms of *bondage* about me, as incoherent discourse, etc., but have now happily regained my freedom. Having now wrote till I have tired my hand, and probably thine eyes in reading, I shall conclude with my love to Cousin Ruth and thyself and to all enquiring friends,

"JOHN DALTON."

Again he writes: —

"I may here observe that it has been my lot for three years past to be daily gaining acquaintance of both sexes. I have consequently had opportunities of estimating and comparing characters upon a pretty extensive scale. Since my first introduction to ——, twelve months ago, I have

spent a day or two with them at six different intervals, with the highest satisfaction, as I have never met with a character so finished as Hannah's. What is called strength of mind and sound judgment she possesses in a very eminent degree, with the rare coincidence of a quick apprehension and the most lively imagination. Of sensibility she has a full share, but does not affectedly show it on every trivial occasion. The sick and the poor of all descriptions are her personal care. Though undoubtedly accustomed to grave and serious reflections, all pensiveness and melancholy are banished from her presence, and nothing but cheerfulness and hilarity diffused around. Her uncommon natural abilities have been improved by cultivation, but art and form do not appear at all in her manner: all is free, open, and unaffected. Extremely affable to all, though everyone sees and acknowledges her superiority, no one can charge her with pride. She is, as might be expected, well pleased with the conversation of literary and scientific people, and has herself produced some essays that would do credit to the first geniuses of the age, though they are scarcely known out of the family, so little is her vanity. Her person is agreeable, active, and lively. She supports conversation, whether serious, argumentative, or jocular, with uncommon address. In short, the *tout ensemble* is the most complete I ever beheld. Next to Hannah, her sister Ann takes it, in my eye, before all others. She is a perfect model of personal beauty. I do not know one that will bear a comparison with her in this respect — at least in our society. With abilities much superior to the generality, she possesses the most refined sensibility, but in strength of mind and vigour of understanding must yield to her elder sister. I dwell with pleasure upon the character of these two amiable creatures, but would not have thee communicate my sentiments to others."

From the age of twelve up to his death Dalton earned his living as a schoolmaster, but not an ordinary one; for having set his pupils to their

lessons, and having given them a hint how to proceed, he would leave them pretty much alone, believing in the doctrine, which he had practised throughout his life, that self-education is the only true one. Thus he found himself at liberty to look after his own experimental work, or to make the calculations which they entailed. Inheriting the spirit of his frugal and economical forbears, he always had enough for his simple needs; indeed, when sixty years of age, he considered himself a moneyed man. To begin with, he charged only 1s. per lesson, after a while he increased his fee to 1s. 6d., and ultimately demanded and obtained the maximum of 2s. 6d. per hour! One day Sir James Bardsley, a well-known Manchester physician of the day and a friend of Dalton's, on calling upon him noticed half-a-crown lying on the table. "You throw your money about carelessly," said he. "Ay," answered Dalton, "a woman has just gone away that I have been teaching a bit of arithmetic to, and thou sees she has left me half-a-crown." His diary abounds with passages showing his scrupulous care about money matters, and also his interest in getting his money's worth: "Mr. J. Pearson and I walked to Hayfield (four miles), breakfasted there on tea, two basins of milk, four eggs, bread and butter, muffins, etc.; for what? for 9d. apiece!" But besides his ordinary tutorial work Dalton taught pupils in his laboratory, and for this he charged the exorbitant sum of 3s. 6d. He also acted as a professional or

consulting chemist, and earned half-a-guinea now and again for a water analysis, for which at the present day the charge would be twenty times as much. Then he frequently gave courses of lectures, as we shall see, not only in Manchester, but in London, Edinburgh, and elsewhere.

We now pass on to trace the development of the greatest work of his life—the discovery of the laws of Chemical Combination and the foundation of the Atomic Theory.

CHAPTER VI.

DALTON'S ATOMIC THEORY.

ALTHOUGH this memoir does not profess to be a history of the Atomic Theory concerning the constitution of matter from the earliest ages to the time of Dalton, but has a much less ambitious aim — namely, to give an account of the life and work of the great Manchester chemist, and of the Atomic Theory of chemistry as developed by him — it may be well in a few sentences to call to mind what had been thought and done, as regards this subject, by others before his time. From very early times, even up to the present, two opposing views have been held respecting the constitution of matter. The one of these is the atomic view, the other the dynamic. According to the first of these, matter is divisible into minute, indestructible, unchangeable particles called atoms (*a* privative, τεμνω, *I cut*). According to the second, no limit is placed on the divisibility of matter, and therefore no finite particles exist, and all we observe is explained by attractions or repulsions. The one

involves the discontinuity of matter, the other involves its continuity.

The atomic view was held not only by the Greek and Roman philosophers, but also by the wise men of the East in Egypt and India. And the conviction that matter was not of haphazard constitution, but was arranged according to definite laws, appears to have been long borne in upon the human mind, and is expressed in the well-known words: "God ordered all things by measure, number, and weight."

Newton was a staunch upholder of the Atomic Theory. "It seems," * says Newton, "probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties, and in such proportion, as most conduced to the end for which He formed them; and that these primitive particles, being solids, are incomparably harder than any porous bodies compounded of them; even so very hard as never to wear or break in pieces, no ordinary power being able to divide what God Himself made one in the first creation. While the particles continue entire they may compose bodies of one and the same nature and texture in all ages; but should they wear away, or break in pieces, the nature of things depending on them would be changed. Water and earth composed of old worn

* Horsley's "Newton," vol. iv. p. 260. Quoted by Dalton.

particles and fragments would not be of the same nature and texture with water and earth composed of entire particles in the beginning. And therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various situations, and new associations and motions of these permanent particles, compound bodies being apt to break, not in the midst of solid particles, but where those particles are laid together and only touch at a few points. . . . God is able to create particles of matter of several sizes and figures, and in several proportions to the space they occupy, and perhaps of different densities and forces. . . . At least, I see nothing of contradiction in all this. . . . Now, by the help of these principles, all material things seem to have been composed of the hard and solid particles above mentioned — variously associated, in the first creation, by the counsel of an intelligent agent."

It is, however, to Dalton, and to him alone, that the honour of founding a *chemical* atomic theory is to be ascribed. It was he who first explained the *facts* of chemical combination by a *theory* which has stood the test of time, and is not contradicted by any known phenomenon of chemical action. Dalton, it must be here remarked, never pretended — nor, indeed, has it ever been urged on his behalf by others — that he was the first to propose an atomic theory of matter. He knew quite well that such views had long been the property of mankind. "These observations," he says,

"have tacitly led to the conclusion which seems universally adopted, that all bodies of sensible magnitude, whether solid or liquid, are constituted of a vast number of extremely small particles bound together by a force of attraction," etc.

In the preceding chapters it has been pointed out that Dalton's mind was of a corpuscular turn. He was thoroughly saturated with the Newtonian doctrine of atoms; he was in the habit, from his early days, of looking at things from an atomic point of view. He was never tired of explaining that "homogeneous elastic fluids are constituted of *particles*." It was in his volume on Meteorology that he insisted on the separate existence of aqueous vapour from the other constituents of the air, and this was the first germ of his Atomic Theory, because he viewed the gases as consisting of independent atoms.

There has been a considerable amount of discussion as to how Dalton arrived at those important conclusions, and various surmises on the subject have from time to time been made. The matter is now set at rest for ever by Dalton himself. Amongst the "Dalton papers" in the possession of the Manchester Literary and Philosophical Society I have been fortunate enough to find the manuscript notes prepared by Dalton for the course of lectures delivered in the winter of 1809–10, at the Royal Institution, Albemarle Street. These notes are of the greatest interest, because they give his own explanation of the genesis of

his ideas regarding the Atomic Theory of chemistry, and as they have not been hitherto published, I need scarcely apologise for now presenting them *verbatim*. From what follows it will be clear that it was the application of the principle of the Newtonian atom to the constitution of the gases contained in the atmosphere that led Dalton to his Atomic Theory.

The printed syllabus of the seventeenth lecture, delivered on Jan. 27th, 1810, is as follows:—

"Saturday, January 27th, at 2 o'clock. Mr. Dalton, Natural Philosophy. Lecture 17.*

"*Chemical Elements.*—Divisibility of matter considered. Elastic fluids exhibit matter in extreme division. Other bodies constituted of atoms as well as elastic fluids. All atoms of the same matter alike in weight, bulk, etc. Bodies deemed simple until they are decomposed. Chemical synthesis considered. Tables of arbitrary marks representing the Elements."

For this lecture, Dalton's manuscript notes are as follows:—

"As the ensuing lectures on the subject of the *Chemical Elements* and their combinations will perhaps be thought by many to possess a good deal of novelty, as well as importance, it may be proper to give a brief historical sketch of the train of thought and experience which led me to the conclusions about to be detailed. Having been long accustomed to make meteorological observations, and to speculate upon the nature and constitution of the atmosphere, it often struck me with wonder how a *compound* atmosphere, or a mixture of two or more elastic fluids, should constitute apparently a homogeneous mass,

* The course consisted of twenty lectures on mechanics, pneumatics, etc., and Dalton's first lecture had been delivered on December 21st, 1809.

or one in all mechanical relations agreeing with a simple atmosphere.

"Newton had demonstrated clearly in the 23rd Prop. of Book II. of the 'Principia' that an elastic fluid is constituted of small particles or atoms of matter which repel each other by a force increasing in proportion as their distance diminishes. But modern discoveries having ascertained that the atmosphere contains three or more elastic fluids of different specific gravities, it did not appear to me how this proposition of Newton's would apply to a case of which he, of course, could have no idea. The same difficulty occurred to Dr. Priestley, who discovered this compound nature of the atmosphere. He could not conceive why the oxygen gas, being specifically heaviest, should not form a distinct *stratum* of air at the bottom of the atmosphere, and the azotic gas one at the top of the atmosphere. Some chemists upon the Continent — I believe, the French — found a solution of this difficulty (as they apprehended). It was *chemical affinity.* One species of gas was held in solution by the other; and this compound in its turn dissolved water — hence *evaporation,* rain, etc. This opinion of air dissolving water had long before been the prevailing one, and naturally paved the way for the reception of that which followed — of one kind of air dissolving another. It was objected that there was no decisive *marks* of chemical union when one kind of air was mixed with another. The answer was, that the affinity was of a very slight kind, not of that energetic cast that is observable in most other cases. I may add, by-the-bye, that this is now, or has been till lately, I believe, the prevailing doctrine in most of the chemical schools in Europe. In order to reconcile — or, rather, adapt — this chemical theory of the atmosphere to the Newtonian doctrine of repulsive atoms or particles, I set to work to combine my atoms upon paper. I took an atom of water, another of oxygen, and another of azote, brought them together, and threw around them an atmosphere of heat, as per diagram. I repeated the operation, but soon found that the watery particles were exhausted (for they make but

a small part of the atmosphere). I next combined my atoms of oxygen and azote one to one; but I found in time my oxygen failed. I then threw all the remaining particles of azote into the mixture, and began to consider how the general equilibrium was to be obtained. My triple compound of *water*, *oxygen*, and *azote* were wonderfully inclined, by their superior gravity, to descend and take the lowest place. The double compounds of *oxygen* and *azote* affected to take a middle station; and the azote was inclined to swim at the top. I remedied this defect by lengthening the wings of my heavy particles — that is, by throwing more heat around them, by means of which I could make them float in any part of the vessel. But this change, unfortunately, made the whole mixture of the same specific gravity as azotic gas. This circumstance would not for a moment be tolerated. In short, I was obliged to abandon the hypothesis of the chemical constitution of the atmosphere altogether as irreconcileable to the phenomena. There was but one alternative left — namely, to surround every individual particle of *water*, of *oxygen*, and of *azote* with heat, and to make them respectively centres of repulsion, the same in a *mixed* state as in *simple* state. This hypothesis was equally pressed with difficulties, for still my oxygen would take the lowest place, my azote the next, and my steam would swim upon the top. In 1801 I hit upon an hypothesis which completely obviated these difficulties. According to this, we were to suppose that atoms of one kind did *not* repel the atoms of another kind, but only those of their own kind. This hypothesis most effectually provided for the diffusion of any one gas through another, whatever might be their specific gravities, and perfectly reconciled any mixture of gases to the Newtonian theorem. Every atom of both or all the gases in the mixture was the centre of repulsion to the proximate particles of its own kind, disregarding those of the other kind. All the gases united their efforts in counteracting the pressure of the atmosphere, or any other pressure that might be exposed to them.

"This hypothesis, however beautiful might be its

application, had some improbable features. We were to suppose as many distinct *kinds* of repulsive powers as of gases; and, moreover, to suppose that *heat* was not the repulsive power in any one case — positions certainly not very probable. Besides, I found from a train of expts. which have been published in the 'Manchester Memoirs' that the diffusion of gases through each other was a *slow* process, and appeared to be a work of considerable effort.

"Upon considering this subject, it occurred to me that I had never contemplated the effect of *difference of size* in the particles of elastic fluids. By *size** I mean the hard particle at the centre and the atmosphere of heat taken together. If, for instance, there be not exactly the same *number* of atoms of oxygen in a given volume of air as of azote in the same volume, then the *sizes* of the particles of oxygen must be different from those of azote. And if the *sizes* be different, then — on the supposition that the repulsive power is heat — no equilibrium can be established by particles of unequal sizes pressing against each other. (*See* diagram.)

"This idea occurred to me in 1805.† I soon found that the *sizes* of the particles of elastic fluids *must* be different. For a measure of azotic gas and one of oxygen, if chemically united, would make nearly *two* measures of nitrous gas, and those *two* could not have *more* atoms of nitrous gas than the *one* measure had of azotic or oxygen. (*See* diagram.) Hence the suggestion that all gases of different kinds have a difference in the *size* of their atoms; and thus we arrive at the reason for that diffusion of every gas through every other gas, without calling in any other repulsive power than the well-known one of heat. This, then, is the present view which I have of the constitution of a mixture of elastic fluids."

With these introductory remarks, Dalton proceeded to illustrate the several points referred to in

* By "size" he perhaps includes the idea of *weight*.

† Dalton seems here to have mistaken the date, for in the autumn of 1803 he gave a table of the relative weights of the ultimate atoms (*see* page 106).

the syllabus. The notes he made for his own guidance are as follows: —

"1. Divisibility of matter considered. Atoms. (*See* Newton's Ideas.)

"2. Elastic fluids exhibit matter in extreme division. Newton, B. 2, Prop. 23. (*See* diagram.)

"Hydrogen and oxygen cannot be broken down into finer kinds by electricity, like flour, etc.

"Compound gases, as nitrous, carbonic acid, are separated into their ulterior elements by electricity. (*See* diagram — atmosphere.)

"3. Other bodies constituted of atoms as well as elastic fluids — charcoal, sulphur, phosphorus. Metals, by combining with atoms of elastic fluids, shew that they have atoms.

"4. All atoms of the *same kind* alike in wt., bulk.

"5. Atoms of different kinds unequal in wt., etc. (*See* Newton.)

"6. Bodies deemed simple till they are decomposed.

"7. Chemical synthesis. Exhibit two particles.* (*See also* Newton.)

"8. Table of arbitrary marks.

"9. Gay-Lussac's notion." †

Dalton concluded this lecture with the following words: —

"The different *sizes* of the particles of elastic fluids under like circumstances of temperature and pressure being once established, it became an object to determine

* The writer remembers a pupil, to whom he had explained the Atomic Theory by help of wooden blocks, giving the following answer to the question, "Describe Dalton's Atomic Theory:" — "Dalton's Atomic Theory consists of cubical blocks of wood painted various colours."

† It must have been interesting to have heard Dalton's remarks on this "notion."

the relative *sizes* and *weights*, together with the relative *number*, of atoms in a given volume. This led the way to the combinations of gases, and to the *number* of atoms entering into such combinations, the particulars of which will be detailed more at large in the sequel. Other bodies besides elastic fluids — namely, liquids and solids — were subject to investigation, in consequence of their combining with elastic fluids. Thus a train of investigation was laid for determining the *number* and *weight* of all chemical elementary principles which enter into any sort of combination one with another."

"LECTURE 18, Jan. 30th, 1810. CHEMICAL ELEMENTS.

"SYLLABUS: — Combination of Simple Atoms, constituting Compound Atoms. — Manner of finding the relative Weights of Atoms. — Arrangement of three or more Atoms forming one Compound — of Water, of Ammonia, of the various Compounds of Azote and Oxygen.

"In the last lecture we endeavoured to shew that matter, though divisible in an *extreme degree*, is nevertheless not infinitely indivisible — that there must be some point beyond which we cannot go in the division of matter. The existence of these ultimate particles of matter can scarcely be doubted, though they are probably much too small ever to be exhibited by microscopic improvements.

"I have chosen the word *atom* to signify these ultimate particles in preference to *particle, molecule*, or any other diminutive term, because I conceive it is much more expressive; it includes in itself the notion of *indivisible*, which the other terms do not. It may, perhaps, be said that I extend the application of it too far when I speak of *compound atoms;* for instance, I call an ultimate particle of *carbonic acid* a *compound atom.* Now, though this atom may be divided, yet it ceases to become carbonic acid, being resolved by such division into charcoal and oxygen. Hence I conceive there is no inconsistency in speaking of compound atoms, and that my meaning cannot be misunderstood.

"It has been imagined by some philosophers that all

matter, however unlike, is probably the same thing, and that the great variety of its appearances arises from certain powers communicated to it, and from the variety of combinations and arrangements of which it is susceptible. From the notes I borrowed from Newton in the last lecture this does not appear to have been his idea. Neither is it mine. I should apprehend there are a considerable number of what may properly be called *elementary* principles, which never can be metamorphosed one into another by any power we can controul. We ought, however, to avail ourselves of every means to reduce the number of bodies or principles of this appearance as much as possible; and, after all, we may not know what elements are absolutely indecomposable and what are refractory, because we do not apply the proper means for their reduction. We have already observed that all *atoms of the same kind*, whether simple or compound, must necessarily be conceived to be alike in shape, weight, and every other particular."

"LECTURE 19, Jan. 31st, 1810. CHEMICAL ELEMENTS.

"SYLLABUS: — Compounds of Charcoal and Oxygen. — Carbonic Oxide. — Carbonic Acid. — Compounds of Charcoal and Hydrogen. — Olefiant Gas. — Sulphur, Phosphorus, and their Compounds. — Earths. — Metals. — Metallic Oxides and Sulphurets.

"In the preceding investigations on the number and weights of the elementary principles constituting *water*, *ammonia*, and the various compounds of azote and oxygen, you will have remarked that the conclusions were derived principally from the facts and experience of others, without any additional facts of my own discovery that merit particular notice.

"The composition and decomposition of water had been ascertained by British and foreign chemists; that of ammonia by Berthollet and several others. The compounds of azote and oxygen had been successively developed by Cavendish, Priestley, Davy, and others. I may, however, observe that the *nitrous compounds* have occupied a great portion of my time and attention at different seasons. The elegant and instructive experiments on the

effect of electricity on nitrous gas deserve notice. By electrifying nitrous gas over water, in a short time one hundred measures are reduced to twenty-four which upon examination are pure azote.

"1. Theory of it explained.

"2. Theory of the formation of nitric acid in Mr. C.'s (Cavendish) experiment.

"The simple and easy method of combining the *least* portion of oxygen with the *greatest* of nitrous gas, which I pointed out in the last lecture, was the result of my own investigation, and affords a convincing proof of the real nature of what is called *nitrous acid*, which is constituted of one atom of oxygen united to two of nitrous gas (*see* Figure).

"From the preceding remarks it will be perceived that I advanced thus far in my theoretic progress without meeting with much obstruction. The way had been paved by others. But when I directed my views to the compounds of charcoal and oxygen, and charcoal and hydrogen, I found that all the commonly received doctrines were adverse to my proceeding and irreconcileable with my views.

"Mr. Tennant's experiments in the Philos. Transact., 1797, had shewn the identity of diamond and charcoal in a chemical point of view; but the succeeding experiments of Guyton Morveau, on the combustion of diamond, supplanted the former in the judgement of a great part of our chemists; diamond was concluded to be a simple body, and charcoal the oxide of diamond. Mr. Cruickshanks soon after discovered the gas called carbonic oxide. The doctrine of the compounds of charcoal, or rather diamond and oxygen, then stood thus:

	Parts.	
Diamond . . .	18	
Oxygen . . .	10	
	28	Charcoal.
Oxygen . . .	41	
	69	Carbonic oxide.
Oxygen .	31	
	100	Carbonic acid.

"A very little reflection convinced me that the doctrine of charcoal being an oxide of diamond was highly improbable; and experience confirmed the truth of Lavoisier's conclusion, that 28 parts charcoal + 72 oxygen constitute carbonic acid; also that carbonic oxide contained just half the oxygen that carbonic acid does, which, indeed, had been determined by Clement and Desormes, two French chemists, who had not, however, taken notice of this remarkable result."

In the last lecture of this course, Dalton explained his singular and, as it turned out, altogether erroneous views respecting the chemical nature of chlorine, hydrochloric, and hydrofluoric acids.

"LECTURE 20, February 3rd, 1810. CHEMICAL ELEMENTS.

"SYLLABUS:—Fluoric, Muriatic, Oxymuriatic, Hiperoxymuriatic and Acetic Acids.—Weights of the Component Parts of Neutral Salts from Theory and Experiment.—Action of Common Electricity on Compound Gases and Gaseous Mixtures.—Conclusion of the Course.

"When we consider the very important part which the two elements of *hydrogen* and *oxygen* seem to perform in the arrangement of chemical compounds, we are inclined to wonder that no more than *one* compound of these two elements themselves should be found.

"*Water*, that most beneficial essential of all liquids, is formed of oxygen and hydrogen. Besides this one, there is not a compound of these two elements generally known and recognised as such. It is singular if we have not somewhere a principle consisting of *two* atoms of oxygen and *one* of hydrogen, or *two* of hydrogen and *one* of oxygen. The former of these ought to be an *acid*, conformably to what we observe in other similar cases. The latter ought to be a combustible gas. All the other common elements, *azote*, *charcoal*, *sulphur*, and *phosphorus*, combine each one with two atoms of oxygen to form acids. Why

should not hydrogen do the same? This question has been frequently put, but no satisfactory answer has been given. Upon comparing the results of experience, and applying the theoretic views which I have been endeavouring to develope, it appears to me *very probable*, at least, that the acids denominated *fluoric* and *muriatic*, with derivatives, are constituted of the elements of hydrogen and oxygen, and are, in reality, the very compounds of which we have just been hinting. I would not, however, be understood to mean that these views are the necessary result of the Atomic Theory, and that its truth or falsehood depends upon the determination of the question. From the want and imperfection of facts relating to these subjects, nothing, perhaps, decisive can be yet advanced. I intend to point out such reasons and such facts as have induced me to adopt this opinion, and must leave it to others to judge how far they support the probabilities above mentioned."

Then Dalton gives short memoranda of the analytical and other grounds upon which he based his views. He concludes his course in the following words: —

"*Conclusion of the Course.*

"I cannot conclude this course of lectures without expressing my high satisfaction with the general attention that has been given to the subjects under discussion, and with the indulgence which has been given me when adverse circumstances occurred. I shall always associate these grateful impressions with the recollection of the event. To those who feel highly interested themselves in the promotion and extension of science it is a peculiar satisfaction to meet with others of the same description. I shall now return to comparative retirement in order to prosecute the train of enquiry and investigation which I have briefly developed in the late lectures. The results, I am confident, will be found of importance,

and will contribute to establish that beautiful and simple theory of chemical synthesis and analysis which I have adopted from a conviction of its application to the general phenomena of chemistry, and which will in due time, I am persuaded, be made the basis of all chemical reasoning respecting the absolute *quantities* and the proportions of all elementary principles, whether simple or compound.

"R. I., Feb. 3rd, 1810."

CHAPTER VII.

ATOMIC THEORY (*continued*).

FROM the foregoing it is clear that Dalton was a thoroughgoing atomist. He was deeply steeped in Newtonianism. For him the atoms, "hard, impenetrable, moveable," had as actual an existence as if he had seen and handled them. He saw in his mind's eye the atoms of oxygen, of azote, and of watery vapour existing in the air. He drew them on paper, and tried now to combine them so as to account for the homogeneous character of the atmosphere. In this endeavour he failed, and at once had recourse to another hypothesis — viz., that these atoms are not chemically combined, but that the particles of one gas act as vacua to those of another, thus accounting for the laws of diffusion which he had discovered some time previously. Then he steps on to the conclusion that the sizes of the atoms of different gases must be different, and "it became an object to determine their relative sizes and weights." And thus the foundation of the Atomic Theory was laid. But still the superstructure remained to be built up. Here

again we can trace his further progress. Early in 1803, as we have seen, Dalton had found that nitrogen (his azote) and oxygen combine together to form compounds which contain their constituents in simple multiple proportions — viz., in the first case, one atom of nitrogen to one of oxygen; in the second case, two atoms of nitrogen to one of oxygen, and in no intermediate proportions. Dalton doubtless asked himself, Why are these things so? And the explanation was not far to seek. If these gases are made up of small indivisible particles, and if chemical combination consist in the approximation of these particles — the clashing of the atoms as we now term it — then it is clear that when two elementary gases, as oxygen and nitrogen, combine, the simplest form of the compound is that produced by one atom of the first coming into juxtaposition with one atom of his second gas. If any further combination is possible — and that possibility is not foretold by Dalton's theory, but is a matter to be decided by experiment — such a new combination can only be brought about by the juxtaposition of a second atom of one of the combining elements.

In Dalton's Laboratory Notebook for 1802–4, in the possession of the Manchester Society, I find the following entry on Sept. 6th, 1803: —

"Observations on the ultimate particles of bodies and their combinations."

And under this is written —

"CHARACTERS OF ELEMENTS.

○ Hydrogen	⦶ Azote
⊙ Oxygen	● Carbone, pure charcoal
	⊕ Sulphur."

And a few pages further on Dalton gives examples of his method of expressing the composition of chemical compounds, viz.: —

⦶⊙⦶ Nitrous Oxide	○⊙ Water
⦶⊙ Nitrous Gas	⦶○ Ammonia
⊙⦶⊙ Nitric Acid	⊙● Gaseous Oxide of Carbon
⊙⦶⊙ ⦶⊙ Nitrous Acid	⊙●⊙ Carbonic Acid
⊕⊙ Sulphurous Acid	⊙⊕⊙ Sulphuric Acid.

On another page of this notebook, with the date Oct. 12th, 1803, I find the following: —

"New theory of the constitution of the ult. atoms of bodies."

CHARACTERS.					OR THUS.
Ⓗ Hydrogen	...	...	...	...	⊙
Ⓐ Azote ...	...	...	...	...	⦶
○ Oxygen	...	...	...	...	○
● Carbone or Charcoal ...			...	...	●
Ⓢ Sulphur	...	...	...	...	⊕
Phosphorus	...	...	...	...	○

And these are interesting as being the very first attempts to indicate by symbol the atomic constitution of chemical compounds.

But the most important part of Dalton's theory remains to be considered; and it is this part which especially distinguishes it from all previous conceptions of the constitution of matter. It was, as we have seen, "an enquiry into the *relative weights* of the ultimate particles of bodies — a subject," he adds, "as far as I know, entirely new. I have lately been prosecuting this enquiry with remarkable success." The weights of the atoms are then different. The atom of oxygen is — according to Dalton, in the year 1803 — rather more than five times heavier than the atom of hydrogen; that of nitrogen (azote) four times heavier — and so on; but every atom of the same element has the same unalterable weight.

The first published table of atomic weights was that appended to Dalton's memoir on the absorption of gases in water, read before the Society on October 21st, 1803 (*see* p. 106). I find in Dalton's notebook an earlier table dated September 6th in that year, giving numbers differing somewhat from the printed ones. Thus we have: —

Ult. at.	Hydrogen	1	Ult. at.	Nitrous Oxide	13·66
,,	Oxygen	5·66	,,	Nitric acid	15·32
,,	Azote	4	,,	Sulphur	17
,,	Carbon (charcl.)	4·5	,,	Sulphurous acid	22·66
,,	Water	6·66	,,	Sulphuric acid	28·32
,,	Ammonia	5	,,	Carbonic acid	15·8
,,	Nitrous Gas	9·66	,,	Oxide of carbon	10·2

It will be observed that no mention is here made of the two compounds of carbon and hydrogen whose names and compositions are given in the table printed in the above-named memoir. Hence it is clear that, contrary to generally received opinion, Dalton had arrived at conclusions respecting the atomic weights of certain elements and of some of their compounds before the summer of 1804, when, according to his own statement, he carried out a further experimental inquiry into the composition of two compounds of carbon and hydrogen — viz., olefiant gas and light carburetted hydrogen — which led to results fully confirming the truth of his theory. These experiments showed that — as in the case of nitrous oxide and nitrous gas, and in that of carbonic acid and the oxide of carbon, so, too, in the case of "carburetted hydrogen from stagnant water" and olefiant gas — a simple multiple proportion holds good between the weights of the constituent elements; that is, the first of these gases contains just twice as much hydrogen, to the same weight of carbon, as the second (olefiant gas) does; and hence Dalton represented them by the symbols ○●○ and ●○ respectively. This conclusion was, however, not arrived at till 1804, and yet the numbers for the two hydrocarbons (6·3 and 5·3) appear in the table attached to a memoir read at the end of October, 1803 (*see* p. 107). The explanation of this is that the volume of memoirs was not published till November, 1805, and that in the meantime Dalton made additions

and alterations to the table. "It was in the summer of 1804," says Dalton, "that I collected at various times and in various places the inflammable gas (marsh gas) obtained from ponds." Those who wish to see how Dalton did this should visit the Manchester Town Hall and inspect the wonderful fresco by Madox Brown, where the process is clearly indicated.

Chemists nowadays do not employ the circular symbols used by Dalton, but have adopted what they consider to be a much simpler mode of expression. This consists in using the first letter of the name of the element (sometimes the modern, sometimes the Latin, name) with a small number placed below to indicate the number of atoms of that element occurring in the compound when more than one atom is present. Thus Dalton's two hydrides of carbon would be CH_2 and CH, or, as we now write them, CH_4 and C_2H_4. This modern system was introduced by the Swedish chemist, Berzelius. Each letter carries with it, understood as it were, a given number. Thus H does not signify any given weight of hydrogen, but always 1 part by weight, and so for all the other elements, each having its own special atomic weight understood. And so, too, for their compounds. Thus the formula of a chemical compound not only indicates of what elements it is composed, but tells us also how much of each element is contained in it. The language of chemistry is, therefore, not merely qualitative, but also quantitative.

It is a singular trait of Dalton's mind that he could not bring himself to adopt — or, if one may say so, even to understand — this system of chemical nomenclature, apparently so simple and effectual. On the contrary, he considered it to be unscientific. Writing to Graham so late as 1837, he says: —

"Berzelius's symbols are horrifying: a young student in chemistry might as soon learn Hebrew as make himself acquainted with them. They appear like a chaos of atoms . . . and to equally perplex the adepts of science, to discourage the learner, as well as to cloud the beauty and simplicity of the Atomic Theory."

And Dalton to the end adhered to the symbolic language that he introduced in 1802. That this was so is seen from a facsimile table of "Atomic Symbols" drawn up by him for a lecture delivered in the Manchester Mechanics' Institute on October 19th, 1835, and inserted at the close of this volume.

In his chapter on "Chemical Synthesis" in the "New System," Dalton thus explains the symbolic language which he uses: —

"From the novelty as well as importance of the ideas suggested in this chapter it is deemed expedient to give plates exhibiting the mode of combination in some of the more simple cases. A specimen of these accompanies the first part. The elements or atoms of such bodies as are conceived at present to be simple are denoted by a small circle, with some distinctive mark; and the combinations consist in the juxtaposition of two or more of these. When three or more particles of elastic fluids are combined together in one, it is to be supposed that

the particles of the same kind repel each other and therefore take their stations accordingly."

Dalton's discovery was brought prominently before the world of science not by the author himself, but by an influential professor of chemistry, Dr. Thomas Thomson, of Glasgow. In August, 1804, Thomson spent some days with Dalton in Manchester, and as a result of their conversation Thomson became an ardent disciple of the new doctrine, which he taught at once publicly in his lectures, and then introduced into his text-book published in 1807. In his "History of Chemistry" Thomson says: "Mr. Dalton informed me (1804) that the Atomic Theory first occurred to him during his investigation of olefiant gas and carburetted hydrogen gas, at that time imperfectly understood, and the constitution of which was first developed by Mr. Dalton himself. It was obvious from the experiments which he made upon them that the constituents of both were carbon and hydrogen and nothing else; he found further, that if we reckon the carbon in each the same, then carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas does. This determined him to state the ratios of these constituents in numbers, and to consider the olefiant gas a compound of one atom of carbon and one atom of hydrogen, and carburetted hydrogen of one atom of carbon and two atoms of hydrogen. The idea thus conceived was applied to

carbonic oxide, water, ammonia, etc., and numbers representing the atomic weights of oxygen, azote, etc., deduced from the best analytical results which chemistry then possessed."

From what has been said as to the genesis of Dalton's ideas, it is clear that this statement of Thomson is not quite exact. Dalton had formulated his theory and actually given numbers representing the relative weights of the atoms of certain of the elements and their compounds in the early autumn of 1803, before he had investigated the composition of the two hydrides of carbon, which, according to his own statement, he did not do until the summer of 1804.

Observe that throughout Dalton uses the term *relative* weight. Dalton ninety years ago says nothing about the *absolute* weight of the atoms; and now we can still say nothing. No mortal eye has ever seen, or ever will see, an atom. We can, however, form some notion of their probable size. Thus it has been shown that if a drop of water were to be magnified to the size of the earth, the molecules of water would be larger than small shots, but smaller than cricket balls.

How, then, did Dalton ascertain that the atoms of the different elements, being inconceivably minute, are not of the same weight, but that to each a definite number must be attached expressing how many times that atom is heavier than hydrogen, this element

being taken as the unit because it is the lightest known substance? In the first place, it is clear that these numbers must be obtained by calculation from the results of chemical analysis or synthesis of certain compounds of the elements. But on what principle was this calculation based? The principle or assumption adopted was that when only one compound of two elements is known to exist, that compound consists of one atom of each element. That is the simplest conceivable arrangement, and the relative quantities with which these elements combine to form the compound are the least combining weights of the two elements.

Thus, for example, 100 parts by weight of water had been ascertained to contain $85\frac{2}{3}$ parts of oxygen and $14\frac{1}{3}$ parts of hydrogen, or in the proportion of 1 to 6, so that these numbers were taken by Dalton as representing the relative weight of the atoms of hydrogen and oxygen, water consisting of one of each.

In a similar way the relative weights of the atoms of nitrogen and hydrogen were obtained. Ammonia gas is a compound of these two elements, and was supposed to consist of one atom of each of these elements. Analysis proved that it contains hydrogen and nitrogen in the proportion of 1 to 4·2. Hence this latter number is the atomic weight of nitrogen. Then, again, assuming this number, and knowing the composition of the two oxides of nitrogen, it is easy to calculate

the atomic weight of oxygen in these two gases, and thus to show that oxygen enters into every com bination with a definite fixed weight. This gives 4·9 as the relative weight of the atom of oxygen combined with nitrogen, against 6·0 when combined with hydrogen. The mean of these two numbers is nearly 5·5, a number adopted by Dalton in his first published table. In a similar way the weight of carbon (4·5), which, in the hydrocarbons, combines respectively with one and with two parts by weight of hydrogen, also combines with 5·5 and 2 × 5·5 of oxygen to form carbonic oxide and carbonic acid. The numbers thus obtained represent, then, the relative weights of the atoms, and indicate the proportions by weight with which the chemical elements combine to form definite chemical compounds.

Dalton subsequently made considerable changes in his tables of atomic weights. Thus, in the first part of his celebrated work, "A New System of Chemical Philosophy," published in 1808, we find oxygen put down as 7, instead of 5·66 and 5·5, and carbon as 5, instead of 4·5 and 4·3, as four years previously. And again, in a third table found in the second part of the same work, published two years later, a somewhat different series of numbers has been adopted. This shows that, as time went on, he obtained other and more reliable data for his calculations. His first results were but rude approximations; his subsequent ones were rather more in accord with what we now admit to

be the true figures, although they were still far from exact. Indeed, Dalton expressly states this. "It is not necessary to insist on the accuracy of all these compounds both in number and weight; the principle will be entered into more particularly hereafter, as far as respects individual results."

Nevertheless, in spite of his rough methods of experimentation, Dalton's results stand out the greatest landmarks in our science. His great achievement was that he was the first to introduce the idea of quantity into chemistry. It has been said, and with truth, that the Atomic Theory is almost as old as the hills. True, but no one before Dalton used the theory of atoms to explain chemical phenomena. To him is due the glory of placing the science on a firm basis, by showing that the weights of the atoms of the different elements are not identical, but different, and that combination amongst these elements takes place, if more than one compound be formed of the same elements, in simple arithmetical proportion.

In the case of almost every great scientific discovery, many men's minds have been working in the same direction, and it often becomes a question of interest to discuss how far the acknowledged discoverer had been assisted or even anticipated by those who had gone before him. Such a discussion has been raised in this instance. Some have even asserted that Dalton was a plagiarist, and that the credit of the establishment of a chemical Atomic

Theory belonged to others. This is not the place to discuss the question at length. Those who desire to make themselves fully acquainted with the facts and arguments may turn to the pages of Henry or of Angus Smith, where they will find the whole matter clearly and satisfactorily dealt with. It must suffice here to state that a careful consideration of all the circumstances has led to the conclusion that Dalton arrived independently at his important results, and that whilst others had expressed opinions, and gathered facts to support these opinions, which approached the complete theory, they certainly did not reach it.

The nearest approximation was made by Mr. William Higgins, an Oxford man; and in 1814 he put forward a claim to be the rightful owner of the Atomic Theory, by virtue of the statements contained in a volume published in 1789. It appears certain that Dalton was unacquainted with Higgins's work until several years after he published his "New System," and thus the charge of plagiarism is fully disposed of. Indeed, no one who has appreciated Dalton's character could imagine that he would be guilty of such a proceeding. He was a man of original ideas, and paid little attention to the work of others, often, as we shall see, to the extent of refusing to admit truths which were patent to other minds. Still, it might be that Higgins and others had the priority in conception, and then, in the history of science, Dalton would have to take the second place. Such is, however, not the verdict

of either contemporary, or of later chemists, all the world over. Thus, Thomas Thomson, of Glasgow, who saw at a glance the great importance of Dalton's new doctrine, who cordially adopted it, and who brought it at once not only before his class, but before that distinguished but somewhat *difficile* body, the Royal Society,* says, in a memoir in the "Annals of Philosophy": —

> "I have certainly affirmed that the Atomic Theory was not established in Mr. Higgins' book. I have had that book in my possession since the year 1798, and have perused it carefully; yet I did not find anything in it which suggested to me the Atomic Theory. That a small hint would have been sufficient I think pretty clear from this — that I was forcibly struck with Mr. Dalton's statements in 1804, though it did not fill half an octavo page; so much, indeed, that I afterwards published an account of it, and I still consider myself as the first person who gave the world an outline of the Daltonian theory."

Sir Humphry Davy was much slower than Thomson to adopt Dalton's results. He was full of his discovery of the decomposition of the alkalies, and he writes to Dalton in 1809 that, though he is glad to hear his new views of the atomic system, he "doubts whether we have yet obtained any elements"; and in a letter to Dalton, dated May 25th, 1810, Davy writes:

* In Thomson's paper, published in the Phil. Trans. for 1808, the following words occur: — "This curions theory, which promises to throw unexpected light on the obscurest parts of chemistry, belongs to Mr. Dalton."

"I shall be sorry if you introduce into your rising system an hypothesis which cannot last concerning the alkaline metals." In 1811 he further objects, and says: "I shall enter no further at present into an examination of the opinions, results, and conclusions of my learned friend; I am, however, obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments."

That Dalton was sanguine as to Davy's conversion appears from a letter addressed to Mr. Johns in December, 1809, when Dalton was lecturing in London. "Davy," he says, "is coming very fast to my views on chemical subjects." And he seems to have come at last, for in 1826, when, as President of the Royal Society, Davy presented the Royal Medal to Dalton, he spoke in the warmest terms of his work and of his services to science. But whether Davy was quite converted or not, it is certain that all chemists of later date have not only expressed the opinion that Dalton's claim to the discovery was a just one, but have acknowledged that this theory is one which explains chemical facts, and is accepted by them as the foundation-stone of their science.

Soon after the volume of Dalton's "New System" was in the hands of chemists, a discovery was made in France which, to the minds of almost all, came as a further striking proof of the truth of the Daltonian hypothesis. Strange to say, however, Dalton himself

was about the only man to whose mind the experiments and conclusions of Gay-Lussac did not bring further confirmation of the Atomic Theory. Briefly stated, these were that, under similar circumstances of temperature and pressure, gases combine together in simple proportions by volume. Thus, exactly two volumes of hydrogen and one volume of oxygen gases unite together to form two volumes of water-gas. That Gay-Lussac's results were at once brought before Dalton's notice appears from a letter in the archives of the Manchester Society from Dr. Thomas Thomson to Dalton, dated November 13th, 1809. In this he says: "The most important paper respecting your atomic theory is by Gay-Lussac. It is entirely favourable to it, and it is easy to see that Gay-Lussac admits it, though respect for Berthollet induces him to speak cautiously. His paper is on the combination of gases. He finds that all unite in equal bulks, or two bulks of one to one of another, or three bulks of one to one of another." Then Thomson gives a list of the gaseous combining volumes of different elementary gases as ascertained by Gay-Lussac, and adds that the French chemist declares that Dalton's experiments on this subject are incorrect.

Moreover, it appears from the following letter to his brother Jonathan, dated "12 mo., 11th, 1809," that Dalton had in his possession the very volume which contained Gay-Lussac's celebrated memoir:—

"About two months ago I received a very handsome present from Berthollet, sent me in return for mine sent him. It was 'Memoirs de la Société d'Arcueil,' being the most recent transactions of the Parisian chemists. It contains some very valuable papers. They speak very respectfully of my first part" (of his new system of chemistry).

In a similar strain Berzelius, who at once adopted the Daltonian theory, urges him, in a letter dated from London on October 13th, 1812, to recognise the powerful corroboration of his system effected by the researches of Gay-Lussac. The words of the great Swedish chemist are as follows:—

"Vous avez raison eu ce que la théorie des proportions multiples est une mystère sans l'hypothèse atomistique, et, autant que j'ai pu m'apercevoir, tous les résultats gagnés jusqu'ici contribuent à justifier cette hypothèse. Je crois cependant qu'il y a des parties dans cette théorie, telle que la science vous la doit à présent, qui demandent à être un peu altérées. Cette partie, p. ex., qui vous nécessite de déclarer les expériences de Gay-Lussac sur les volumes des gases qui se combinent pour inexactes. J'aurais cru plutôt que ces expériences étaient la plus belle preuve de la probabilité de la théorie atomistique, et je vous avoue d'ailleurs que je ne croirai pas si aisément Gay-Lussac en defaut, surtout dans une matière où il ne s'agit que de mésurer bien ou mal" (*sic*).

But Dalton remained obdurate, and, as Angus Smith remarks, he does not appear to advantage in the contest in which he ventured to engage with Davy and Gay-Lussac. Dalton, it must be admitted, was not great in exact experimental chemistry. Although

it may be urged that he was self-taught, and began his work when the resources of the experimentalist were scanty and imperfect, yet it is evident that there must have been some inherent deficiency, either in his mind or in his hands, which disqualified him for accuracy in experimentation. "Nature, it would seem, with a wise frugality, averse to concentrate all intellectual excellences in one mind, had destined Dalton exclusively for the lofty rank of a law-giver of chemical science."

The question of how far Dalton supported the principle afterwards advocated by Avagadro — viz., that the number of molecules contained in a given volume is constant for all gases, whether element or compound — has often been debated. Dalton's own words, contained in some manuscript notes for the lectures he delivered in Edinburgh and Glasgow in 1807, leave no room for further question. The third lecture commences as follows: "Elastic fluids: mode of conceiving them (scheme). Are the particles alike in shape, weight, etc.? *Query*, are there the same *number* of particles of any elastic fluid in a given volume and under a given pressure? No; azotic and oxygen gases mixed equal measures give half the number of particles of nitrous gas, nearly in the same volume."

On the enormous progress which atomic chemistry has made since Dalton's time, and especially in the last few years, it is not the province of this volume to

dilate. Suffice it to say that the extraordinary complications in chemical combinations which have been brought to light by modern research all receive their explanation by the application of the principles of the Daltonian Atomic Theory. Without such a theory, modern chemistry would be a chaos, with it, order reigns supreme, and every apparently contradictory and recondite discovery only marks out more distinctly the value and importance of Dalton's work.

In closing this slight sketch of Dalton's greatest discovery, it may be worth while to quote the opinions of a famous Englishman and of a distinguished Frenchman on the life-work of the Manchester philosopher : —

"The extreme simplicity," writes Sir John Herschel, "which characterises the Atomic Theory, and which in itself is an indication, not unequivocal, of its elevated rank in the scale of physical truths, had the effect of causing it to be announced by Mr. Dalton in its most general terms, on the contemplation of a few instances, without passing through subordinate stages of painful inductive assent by the intermedium of subordinate laws. . . . Instances like this, where great, and indeed immeasurable, steps in our knowledge of nature are made at once, and almost without intellectual effort, are well calculated to raise our hopes of the future progress of science, and by pointing out the simplest and most obvious combinations — as those which are actually found to be most agreeable to the harmony of creation — to hold out the cheering prospect of difficulties diminishing as we advance, instead of thickening around us in increasing complexity."

Wurtz, in his "Histoire des Doctrines Chimiques," after indicating the work done by Wenzel and Richter, says: —

"Mais l'interprétation théorique faisait encore défaut. Elle découle des travaux d'un savant anglais qui a doté la science de la conception à la fois la plus profonde et la plus féconde parmi toutes celles qui ont surgi depuis Lavoisier. Au commencement de ce siècle la chimie était professée à Manchester par un homme qui joignait à un amour ardent de la science, cette noble fierté du savant qui sait préférer l'indépendance aux honneurs, et à une vaine popularité la gloire des travaux solides. Ce professeur est Dalton; son nom est un des plus grands de la chimie."

CHAPTER VIII.

DALTON IN LATER LIFE.

DALTON from first to last read no fewer than 116 memoirs and essays to the Literary and Philosophical Society of Manchester. To some of the most important of these allusion has already been made; others of less interest were communicated by him as secretary or president more with a view of filling up a blank evening meeting, than with any more important object.

Many quaint remarks which Dalton made *sotto voce* to the secretaries who sat below the presidential chair have been handed down to posterity. On one occasion, when a very long and stupid paper was being read, the president whispered in a voice loud enough to be heard all over the meeting-room: "Well, this is a very interesting paper for those that take any interest in it." At another meeting, after Dalton had communicated the results of a series of barometrical observations, a military gentleman who was present demanded whether these observations were carried on in a house or in the open air. To which sapient question Dalton asked why he wished to know?

"Because," said the colonel, "I understand that the barometer does not stand at the same height inside as it does outside the house." To which Dalton laconically remarked: "Well, that is the first time I ever heard of such a thing!"

He was not a brilliant exponent of scientific truth, but the originality of his ideas, and the importance of the facts which he had discovered, placed him in the forefront of the scientific men of the age, so that we need not wonder to find, as we have done, that the Royal Institution in Albemarle Street—then, as now, always, like the Athenians, seeking after some new thing—invited Dalton to give a course of lectures; and he first appeared before that critical audience on December 22nd, 1803, lecturing not merely on chemistry, but on mechanics and physics, repeating the course in 1809–10.

To those accustomed to the eloquence of Davy and the refinement of Wollaston, the personal appearance and manner of this plain-spoken Cumberland man was doubtless a matter of surprise, knowing as they did his great reputation. The *Quarterly Review* found in the Manchester chemist food for its somewhat acrid criticism, the writer being in all probability unable to grasp the meaning, much less the importance, of the scientific matter which underlay this rough speech and provincial accent. Doubtless some of his expressions were, as the reviewer remarks, uncouth and scarcely appropriate to the subject.

Thus, for instance, Dalton generally spoke of "the great elements, oxygen, nitrogen, and hydrogen, which pervade all nature, as *these articles*, describing their qualities with far less earnestness than a London linendraper would show in commending the very different *articles* which lie on his shelves."

We may now learn in his own words what he thought and did when visiting the Metropolis more than ninety years ago, for the purpose of delivering his first course of lectures at the Royal Institution.

"2nd month, 1st, 1804.

"Dear Brother, — I have the satisfaction to inform thee that I arrived safe from my London journey last seventh day, having been absent six weeks. It has, on many accounts, been an interesting vacation to me, though a laborious one. I went in a great measure unprepared, not knowing the nature and manner of the lectures in the institution, nor the apparatus. My first was on Thursday, December 22nd (1803), which was introductory, being entirely written, giving an account of what was intended to be done, and natural philosophy in general. All lectures were to be one hour each, or as near as might be. The number attending were from one to three hundred of both sexes, usually more than half men. I was agreeably disappointed to find so learned and attentive an audience, though many of them of rank. It required great labour on my part to get acquainted with the apparatus and to draw up an order of experiments and repeat them in the intervals between the lectures, though I had one pretty expert to help me. We had the good fortune, however, never to fail in any experiment, though I was once so ill-prepared as to beg the indulgence of the audience as to part of the lecture, which they most handsomely and immediately granted me by a general

plaudit. The scientific part of the audience was wonderfully taken with some of my original notices relative to heat, the gases, etc., some of which had not before been published. Had my hearers been generally of the description I had apprehended, the most interesting lectures I had to give would have been the least relished; but, as it happened, the expectations formed had drawn several gentlemen of first-rate talents together; and my eighteenth, on heat and the cause of expansion, etc., was received with the greatest applause, with very few experiments. The one that followed was on *mixed elastic fluids*, in which I had an opportunity of developing my ideas that have already been published on the subject more fully. The doctrine has, as I apprehended it would, excited the attention of philosophers throughout Europe. Two journals in the German language came into the Royal Institution whilst I was there from Saxony, both of which were about half filled with translations of the papers I have written on the subject, and comments on them. Dr. Ainslie was occasionally one of my audience, and his sons constantly. He came up at the concluding lecture, expressed his high satisfaction, and he believed it was the same sentiment with all or most of the audience. I was at the Royal Society one evening, and at Sir Joseph Banks' another. This geutleman I had not, however, the pleasure of seeing, he being indisposed all the time I was in London.

"I saw my successor, William Allen, fairly launched. He gave his first lecture on Tuesday preceding my conclusion. I was an auditor in this case — the first time — and had an opportunity of surveying the audience. Amongst others of distinction the Bishop of Durham was present.

"In lecturing on optics I got six ribands — blue, pink, lilac, and red, green, and brown — which matched very well, and told the curious audience so. I do not know whether they generally believed me to be serious, but one gentleman came up immediately after and told me he perfectly agreed with me: he had not remarked the difference by candlelight."

The following letter to Mr. John Rothwell gives further particulars of the same visit:—

"LONDON, Jan. 10th, 1804.

"I was introduced to Mr. Davy, who has rooms adjoining mine in the Royal Institution. He is a very agreeable and intelligent young man, and we have interesting conversations in an evening. The principal failing in his character is that he does not smoke. Mr. Davy advised me to labour my first lecture; he told me the people here would be inclined to form their opinion from it. Accordingly, I resolved to write my first lecture wholly, to do nothing but to tell them what I would do, and enlarge on the importance and utility of science. I studied and wrote for nearly two days, then calculated to a minute how long it would take me reading, endeavouring to make my discourse about fifty minutes. The evening before the lecture Davy and I went into the theatre. He made me read the whole of it, and he went into the furthest corner; then he read it, and I was the audience. We criticised upon each other's method. Next day I read it to an audience of about 150 or 200 people, which was more than were expected. They gave a very general plaudit at the conclusion, and several came up to compliment me on the excellence of the introductory. Since that time I have scarcely written anything—all has been experiment and verbal explanation. In general my experiments have uniformly succeeded, and I have never once faltered in the elucidation of them. In fact, I can now enter the lecture-room with as little emotion nearly as I can smoke a pipe with you on Sunday or Wednesday evenings."

In 1807 he gave a similar course of lectures in Edinburgh, to which the following is the introduction, found in his manuscript:—

"It may appear somewhat like presumption in a stranger to intrude himself upon your notice in the character I am now assuming, in a city like this, so deservedly

famous for its seminaries of physical science. My apology, however, shall be brief. The field of science is large; it is, therefore, impossible for any individual to cultivate the whole. My attention has been directed for several years past, with considerable assiduity, to the subjects of *heat*, of *elastic fluids*, of the *primary elements* of bodies, and the *mode of their combinations*. In the prosecution of these studies several new and important facts and observations have occurred. I have been enabled to reduce a number of apparently anomalous facts to general laws, and to exhibit a new view of the first principles or elements of bodies and their combinations, which, if established, as I doubt not it will in time, will produce the most important changes in the system of chemistry, and reduce the whole to a science of great simplicity, and intelligible to the meanest understanding. My object in the proposed short course of lectures is to exhibit to you the grounds and reasoning on which I entertain those ideas. I chose Edinburgh and Glasgow in preference to any other cities in Britain because I apprehended the doctrines inculcated would in those cities meet with the most rigid scrutiny, which is what I desire. This is my only apology."

A letter from the northern metropolis, addressed to his friend, the Rev. W. Johns, may also be read with interest: —

"EDINBURGH, April 9th, 1807.

"RESPECTED FRIEND, — As the time I proposed to be absent is nearly expired, and as my views have recently been somewhat extended, I think it expedient to write you for the information of enquirers. Soon after my arrival here I announced my intention by advertisement of handbills. I obtained introduction to most of the professional gentlemen in connection with the college, and to others not in that connection, by all of whom I have been treated with the utmost civility and attention. A class of eighty appeared for me in a few days. My five lectures occupied me nearly two weeks; they were

finished last Thursday, and I was preparing to leave the place and return by Glasgow to spend a week. But several of the gentlemen who had attended the course represented to me that many had been disappointed in not having been informed in time of my intention to deliver a course, and that a number of those who had attended the first course would be disposed to attend a second. I have been induced to advertise for a second, which, if it succeeds, will commence on Wednesday, the 22nd, and be continued daily till the conclusion. This will detain me a week yet; I then set off for Glasgow, where I may be detained a week or more, so that I see no probability of reaching Manchester before the beginning of May, to which I look forward with some anxiety. Hitherto I have been most highly gratified with my journey; it is worth coming a hundred miles merely to see Edinburgh. It is the most romantic place and situation I ever saw; the houses touch the clouds. At this moment I am as high above the ground as the cross on St. James's spire; yet there is a family or two above me. In this place they do not build houses side by side as with you; they build them one upon another — nay, they do what is more wonderful still: they build one street upon another, so that we may, in many places, see a street with the people in it directly under one's feet, at the same time that one's own street seems perfectly level and to coincide with the surface of the earth. My own lodgings are up four flights of stairs from the front street, and five from the back. I have just one hundred steps to descend before I reach the real earth. I have a most extensive view of the sea. At this moment I see two ships, and mountains across the Firth of Forth, at a distance of thirty miles. To look down from my windows into the street at first made me shudder, but I am now got so familiar with the view that I can throw up the window and rest on the wall, taking care to keep one foot as far back in the room as I can to guard the centre of gravity. The walks about Edinburgh are most delightfully romantic. The weather is cold; ice every morning, and we had a thick snow a few days ago. Upon walking

up on to an eminence I observed all the distant hills white, the nearer one speckled. I saw five or six vessels just touching the horizon; they seemed to be about ten or twelve miles off, and their white sails looked like specks of snow on the sea. I saw a dozen or two at anchor in the river, and a most charming view of the Fifeshire hills on the other side of the Firth. Adieu. My best regards to you all. "JOHN DALTON."

Again, from London, December 27th, 1809, when giving another course of lectures (for which his notes are found in Chapter VI.), he writes to Mr. Johns, after some pleasant gossip about his fellow-travellers:—

"On Tuesday I spent the greater part of the day (morning they call it here) with Mr. Davy in the laboratory of the Royal Institution. Sir J. Sebright, M.P., who is becoming a student of chemistry, was present. We had a long discussion. In the evening I walked three miles into the City, to Pickford's, to look after my boxes. I found them there, but as they promised to send them next day I did not take them. They disappointed me. On Wednesday I attended Mr. Bond's lecture on astronomy, and prepared for mine the next day. On Thursday, at two, I gave my first lecture. Mr. Pearson, a former acquaintance, went home with me after the lecture, and we had a long discussion on mechanics. Mr. Davy had invited me to dine with the club of the Royal Society at the Crown and Anchor at five o'clock; but I was detained till near six. I got there and called Davy out—all was over; the cheese was come out. I went, therefore, to the nearest eating-house I could find to seek a dinner. Looking in at a window I saw a heap of pewter plates and some small oblong tables covered with cloths. I went in and asked for a beefsteak. 'No.' 'What can I have?' 'Boiled beef.' 'Bring some immediately.' There was nothing eatable visible in the room, but in three minutes I had placed before me a large pewter plate covered completely with a slice of excellent boiled

beef swimming in gravy, two or three potatoes, bread, mustard, and a pint of porter. Never got a better dinner. It cost me 11½d. I should have paid 7s. at the Crown and Anchor. I then went to the Royal Society and heard a summary of Davy's paper on chemistry, and one of Home's on the poison of the rattlesnake — Sir J. Banks in the chair. Davy is coming fast into my views on chemical subjects. On Friday, as I was preparing for my second lecture, I received a visit from Dr. Roget. On the evening I was attacked with sore throat. I sweated it well in the night with cloathing, but it was bad on Saturday, and I was obliged to beg a little indulgence of my auditors on the score of exertion. However, I got through better than I expected. I kept in on Sunday and Monday, and got pretty well recruited. On Tuesday I had my third lecture, after which I went to dine at a tavern to meet the Chemical Club. There were five of us, two of whom were Wollastou and Davy, secretaries of the Royal Society. Wollaston is one of the cleverest men I have yet seen here. To-day, that is Thursday (for I have had this letter two or three days in hand), I had my fourth lecture. I find several ingenious and inquisitive people of the audience. I held a long conversation to-day with a lady on the subject of rain-gauges. Several have been wonderfully struck with Mr. Ewart's doctrine of mechanical force. I believe it will soon become a prevalent doctrine. I should tell Mrs. J. something of the fashions here, but it is so much out of my province that I feel rather awkward. I see the belles of New Bond Street every day, but I am more taken up with their faces than their dresses. I think blue and red are the favourite colours. Some of the ladies seem to have their dresses as tight round them as a drum; others throw them round them like a blanket. I do not know how it happens, but I fancy pretty women look well anyhow.

"I am very regular with my breakfast, but other meals are so uncertain that I never know when or what. Hitherto I have dined at from two to seven o'clock; as for tea, I generally have a cup between nine and ten,

and, of course, no supper. I am not fond of this way of proceeding. They say things naturally find their level, but I do not think it is the case in London. I sent for a basin of soup the other day before I went to lecture, thinking I should have a good threepenny worth; but I found they charged me one shilling and ninepence for a pint, which was not better than some of our Mary's broth. Of course I could not digest much more of the soup."

During his visit to the Metropolis Dalton had learnt, as we have seen, to appreciate London stout, for he was not a total abstainer, although he seems, from his general avoidance of alcohol when at work in Manchester, to have come practically to the same opinion as von Helmholtz, who found that the slightest quantity of alcohol drove away any chance of his arriving at any new and good scientific idea. However this may be, Dalton was nearly paying dearly for his liking for the beverage, for he was seized with symptoms of lead-poisoning, due to the passage of the porter through leaden pipes in the public-house.

"You may perhaps have heard from Dr. Henry," he writes on Jan. 29th, 1810, "that I have been nearly as ill as formerly—that I have been nearly poisoned since I came here. I had been about three weeks when I discovered it was the porter which produced the effects. I have not had a drop since, and have never had any more of the symptoms." Mr. Ransome, a well-known Manchester surgeon, who, with his father, regularly attended Dalton in later years, states, concerning this lead-poisoning, that during his

visit to the Metropolis Dalton discovered very early the effect of lead on the system, it producing numbness and discomfort in his extremities. He at once ascertained the presence of lead by chemical tests, and was afterwards almost nervous when water was offered to him, until he could ascertain its purity by analysis, or unless he assured himself that it had not come into contact with lead. Dalton's caution was also exemplified in the following story. Once suffering from catarrh, this fear of lead-poisoning being strong upon him, his doctor ordered him a dose of James's powder. Next day the patient was found to be better, and the doctor, as is their wont, attributed the improvement to his prescription. "I do not well see how that can be," said Dalton, "for I kept the powder until I could have an opportunity of analysing it." That he did not pin his faith too firmly on doctors' doses is shown by the fact that he preferred to prescribe for himself. He was in the habit, when troubled with a cold, of making a mixture in a pipkin over his laboratory fire of liquorice, treacle, and vinegar, with an occasional addition of paregoric. Of the virtues of this "simple" he was fond of descanting to his pupils, and doubtless it did as well as many a guinea prescription of the authorised version.

If he struck off his porter he did not find it necessary to do so with his pipe, and he describes with appreciation the habits of Dr. Rees ("Encyclopædia" Rees), whom he visited in the evenings.

"The doctor seems a worthy philosopher of the old school; his evening lucubrations are duly scented with genuine Virginia."

From these excursions to London, Edinburgh, and Glasgow to lecture, and from his summer rambles over the fells and hills of his beloved Lake district, Dalton always turned his steps homewards into what he calls "his comparative obscurity," and to his laboratory, with delight. Only those men who know what laboratory work is, and who enjoy it, and who, like myself, have had the advantage of looking over the MSS. volumes of his laboratory notebooks, can understand or appreciate the extent and value of his labours. Whilst teaching his pupils his mind was ever engaged in working out the problems which experiment suggested. Thus he worked double tides—earning his livelihood and pursuing his original work. In this and in everything else Dalton was most methodical. His days were spent as follows:—He rose at eight o'clock, went at once to his laboratory hard by, with a lantern in the dark winter mornings. There he lighted his fire, and returned, generally late, for breakfast with the Johns family. As soon as this was over he went back to his work, staying in the laboratory till his midday dinner, arriving as a rule when the meal was nearly over. He ate moderately, and drank water only. Then back again till nine o'clock, when he came in to supper, after which he smoked a pipe and retired early to rest.

As has been said, his apparatus and appliances were often of the crudest kind. An inspection of the collection of old penny ink-bottles, of rough home-made thermometers and barometers, of his small balance and weights, and other simple forms of apparatus — preserved with care in the museum of the Manchester Society — indicates better than any description could do the nature of his experimentation. Notwithstanding all this, Dalton in most instances arrived at the true explanation of the phenomena which he investigated. He was the first to introduce the processes, now so generally employed, of volumetric analysis, and constantly used these methods in carrying out his investigations. He, too, was the first to point out the fact, which has since become of great importance, that when certain anhydrous salts are dissolved in water no increase in the volume of the latter is caused, proving that the salt enters into the pores of the water. This discovery, afterwards confirmed and extended by the experiments of Joule and Playfair, was said by Dalton to be "the greatest discovery I know of next to the Atomic Theory."

In the year 1810 Davy proposed to Dalton to offer himself as candidate for election to the Royal Society — that distinguished body never condescending to elect a man as Fellow unless he applies for the honour. Either on the grounds of the fee for election being heavy, or possibly because he did not feel sure how far so influential a Fellow as Davy — still opposed

as he was to the Atomic Theory — might press his case, Dalton refused to be put in nomination, and it was not till 1822 that he was able to place "F.R.S." after his name. Honours came first to him from abroad, for, six years before his election to the first of English scientific societies, the French men of science had shown their appreciation of his work by electing him a corresponding member of the French Academy of Sciences, and this honour he prized highly. Any apparent neglect of his merits on the part of our great Society was, however, soon atoned for by the award to Dalton of the first Royal Medal in 1826, by Sir Humphry Davy, "for the development of the chemical theory of definite proportions, usually called the Atomic Theory, and for his various other labours and discoveries in physical and chemical science"; and in presenting the medal the President used the following words: —

"To Mr. Dalton belongs the distinction of first unequivocally calling the attention of philosophers to this important subject. Finding that in certain compounds of gaseous bodies the same elements always combined in the same proportions, and that when there was more than one combination the quantity of the elements always had a constant relation — such as 1 to 2, or 1 to 3, or to 4 — he explained this fact on the Newtonian doctrine of indivisible atoms; and contended that, the relative weight of one atom to that of any other atom being known, its proportions or weight in all its combinations might be ascertained, thus making the statics of chemistry depend upon simple questions in subtraction or multiplication and enabling the student to deduce an immense number of facts from a few well-authenticated

accurate experimental results. Mr. Dalton's permanent reputation will rest upon his having discovered a simple principle universally applicable to the facts of chemistry, in fixing the proportions in which bodies combine, and thus laying the foundation for future labours respecting the sublime and transcendental parts of the science of corpuscular motion. His merits in this respect resemble those of Kepler in astronomy.

"Mr. Dalton has been labouring for more than a quarter of a century with the most disinterested views. With the greatest modesty and simplicity of character he has remained in the obscurity of the country, neither asking for approbation, nor offering himself as an object of applause. He is but lately become a Fellow of this Society, and the only communication he has given to you is one, compared with his other works, of comparatively small interest. The feeling of the Council on the subject is therefore pure. I am sure he will be gratified by this mark of your approbation of his long and painful labours. It will give a lustre to his character which it fully deserves; it will anticipate that opinion which posterity must form of his discoveries; and it may make his example more exciting to others in their search after useful knowledge and true glory."

In 1818 the Government of the day were about to dispatch an expedition to the Polar regions under the command of Sir John Ross. Dalton was invited to join the expedition, but characteristically refused. The correspondence between the afterwards President of the Royal Society and Dalton is of sufficient interest to justify republication:—

"23, Grosvenor Street, Feb. 12th, 1818.

"My Dear Sir,—You have probably heard of the expedition which is preparing for investigating the polar regions. The Royal Society, charged by the Admiralty

with the scientific arrangements of this voyage, is very desirous of making the most of so interesting an opportunity of investigating many important objects relating to meteorology and the theory of the earth.

"They have obtained from the Admiralty the power of recommending a natural philosopher to go this expedition; and it has occurred to me that if you find your engagements and your health such as to enable you to undertake the enterprise, no one will be so qualified as yourself. Probably £400 or £500 a year will be allowed during the voyage. It may last from one or two years, but most probably only three or four months; in all events, it is believed that the whole year's salary will be allowed.

"This plan may not in any way fall in with your views. At all events, I am sure you will not be displeased with me for inquiring if such a proposition will be acceptable to you, and impute this letter to the high respect I have for your talents and acquirements, and the desire that they may be brought into activity, and estimated and rewarded as they ought to be. — I am, my dear sir, very sincerely yours,

"H. Davy."

To this letter Dalton replied immediately as follows: —

"Respected Friend, — In reply to your favour of the 12th inst., I may observe that the novelties of an enterprise similar to the intended one can hardly fail to present much gratification to individual curiosity, and many interesting opportunities for observation of natural phenomena, particularly of meteorology, which certainly operate as strong inducements for me to engage in it; but, on the other hand, to quit the regular habits of a sedentary life for a seafaring one, and that on a voyage of uncertain duration, and of more than ordinary risk, with the prospect of a cold or, at least, a desultory climate; add to these the interruption of my chemical investigations, which I have not yet been able to close — these together outweigh with me any inducement which the proposed scheme can

offer. I must therefore decline the acceptance of the proposition.

"At the same time, I would have it to be understood that I feel gratified by the communication of your favourable opinion of my scientific labours; and it will afford me great pleasure if I can in any way contribute to the successful issue of the expedition, especially as far as it may be connected with the promotion of science.—I remain, with high esteem,

"Your obliged friend,

"JOHN DALTON.

"Sir Humphry Davy, 23, Grosvenor Street, London."

In 1822 Dalton visited Paris and made the personal acquaintance of the numerous French men of science with whom he had corresponded, and with whose work he was familiar. The notes of this journey are, unfortunately, brief; but they are sufficient to enable the reader to form a pleasing picture of the cordial and courteous reception which the English man of science met with at the hands of his scientific brethren in Paris—a reception, both then and since, always accorded to everyone who labours for the cause of scientific progress.

"Saturday, July 6th.—Received a visit from two Swedish chemists from Abo, in Finland, pupils of Berzelius, Bonsdorf, and Nordenskiold. Visited the Venerable Abbé Gregoire. 7th (Sunday).—Attended the service at the British Ambassador's Chapel. From one to two hundred present, chiefly English, and more than half ladies. Very genteel and attentive congregation. Good sermon, well calculated for Paris, on the evidences of Christianity. After 4 p.m. took coach with companions for Arcueil, to dine by invitation with the Marquis Laplace

and lady. Met Berthollet, Biot and lady, Fourier, etc. etc. A most agreeable and interesting visit, and a beautiful place. Monday, 8th July. — Walked down to the arsenal; saw Gay-Lussac for half an hour; went to the Jardin du Roi, saw the wild beasts and the anatomical preparations, etc.; took coach home, and then went to the Institute. About one hundred persons present. Was introduced by Biot and placed in the square adjacent to the officers; was announced by Gay-Lussac (as president) as a corresponding member (English) present. The sitting was from three to five o'clock.* After my announcement my two companions were introduced to the same bench during the sitting. Sunday, 14th. — Gay-Lussac and Humboldt called and spent an hour on meteorology, etc. Took a coach to Thénard's; breakfast *à la fourchette* with him, family, and Dr. Milne-Edwards. Went to the laboratory near M. Biot's and saw a full set of experiments on the deutoxide of hydrogen, most curious and satisfactory. M. Thénard then went with us through the laboratory, showed us the new theatres for chemistry, physique, etc., and then went to M. Ampère's, who had previously prepared his apparatus for showing the new electro-magnetic phenomena. Saw a set of these experiments which, with the aid of Dr. Edwards, were made intelligible to me. 15th. — Took coach to the Arsenal; spent an hour with Gay-Lussac in his laboratory; saw his apparatus for specific gravity of steam, vapours, etc., also M. Welters, the improver of chemical distillation, etc. Walked to the Jardin du Roi, *déjeûner à la fourchette* with Monsieur and Madame Cuvier and youngest daughter. M. Cuvier went

* Dr. Robert Knox was present at the *séance*, and told Dr. Lonsdale that on Mr. Dalton's name being announced the president (Gay-Lussac) and the other members of the Institute rose from their seats and bowed to the Manchester philosopher. Such honours, it was remarked at the time, were not offered to Napoleon le Grand when he took his seat among the renowned "Forty" of France. He remembered in the year 1838 seeing Lord Brougham enter the Institute, without, however, eliciting any special mark of attention from its members.

with us to the museum, and accompanied us for some time, and then left a gentleman to attend us through the museum, being himself engaged, but occasionally meeting us; spent two hours in the museum — the most splendid exhibition of the kind in the universe — it beggars description. Left after two, and took a coach to the Institute; took a cup of coffee, etc., and then entered the library; saw and spoke MM. Milne-Edwards, Biot, Cuvier, Laplace, Berthollet, Brequet, etc.; entered the Institute, heard papers by Milne-Edwards, Biot (on 'The Zodiac de Denderah'), Fourier (on 'The Population of Paris'), after which notice was given for strangers to withdraw, when Gay-Lussac called to me to stay, if I chose, being a member, which I did. The business was about the election of members, and lasted nearly half an hour, after which we broke up. Saw M. Pelleton on coming out, who kindly inquired of me my health, etc.; went with Vanquelin in a coach to dine, when my companions met me; saw M. Payen, a young chemist of promise."

Mr. Dockray, one of Dalton's companions, gives the following interesting account of the reception of the Manchester chemist by the philosophers at Arcueil: —

"At four in the afternoon, by a coach with Dalton to Arcueil, Laplace's country-seat, to dine. On alighting, we were conducted through a suite of rooms, where, in succession, dinner, dessert, and coffee-tables were set out; and onwards through a large hall, upon a terrace, commanding an extent of gardens and pleasure grounds. It is in these grounds that are still remaining the principal Roman works near Paris, the vestiges of Julian's residence as Governor of Gaul. Avenues, parterres, and lawns, terraces, and broad gravel-walks, in long vistas of distance, are bounded by woods, and by higher grounds. As yet we had seen no one, when part of the company came in view at a distance — a gentleman of advanced

years, and two young men. Was it possible not to think of the groves of the Academy and the borders of the Ilyssus? We approached this group, when the elderly gentleman took off his hat, and advanced to give his hand to Dalton. It was Berthollet. The two younger were Laplace's son and the astronomer-royal, Arago. Climbing some steps upon a long avenue, we saw, at a distance, Laplace walking uncovered, with Madame Biot on his arm, and Biot, Fourier, and Courtois, father of the Marchioness Laplace. At the front of the house this lady and her granddaughter met. At dinner, Dalton on the right hand of Madame Laplace, and Berthollet on her left, etc. Conversation on the zodiac of Denderah and Egypt (Berthollet and Fourier having been in Egypt with Napoleon), the different eras of Egyptian sculpture, the fact that so little at Rome — of public buildings — is earlier than Augustus, etc. After dinner, again abroad in the beautiful gardens, and along the reservoir and aqueduct of Julian. These ancient works, after falling very much into decay, were restored by Mary de Medicis. Dalton walking with Laplace on one side and Berthollet on the other I shall never forget."

"The enjoyment and advantage of his stay in Paris," says Dr. Henry, "were greatly enhanced by the friendly attentions of Dr. Milne-Edwards, who kindly acted as interpreter between him and those of the French *savants* who did not speak English." Dalton was always accustomed to mention Dr. Milne-Edwards in terms of grateful regard, and appears to have maintained some intercourse with him by correspondence.

Dalton was, as has been said, not insensible to female charms. Whilst in Paris, he was chaperoned by Mlle. Clémentine Cuvier, the only child of her

distinguished father. He regarded her as one of the most agreeable and attractive young women he had ever met, and deeply lamented her early death. One day he said to a friend, "Ah! she was a bonny lass — she treated me like a daughter."

CHAPTER IX.

DALTON'S PERSONAL CHARACTERISTICS. — LAST YEARS AND HONOURS.

An anecdote told of Dalton's presentation to King William the Fourth is so characteristic of his sturdy, uncompromising habit of mind and his out-spokenness, that it gives the key to the inward man more directly than any long disquisition could well do. He was, as we have seen, in the habit of visiting, at regular intervals, his old Cumbrian friends. Amongst these, one whom he specially favoured was Mr. Wilson Fletcher of Cockermouth. On one occasion, after Dalton's return from London, where he had seen the king and the splendour of St. James's, he came to visit his northern friend, and Mr. Fletcher asked him what he thought of the king, and what he had said. Dalton replied that the king asked him, "Well, Dr. Dalton, how are you getting on in Manchester — all quiet, I suppose?" to which he said, "Well, I don't know — just middlin', I think." To which Mr. Fletcher, laughing at the doctor's simplicity, added, "Why, John, thou hardly showed Court manners in addressing the king in such common parlance."

Dalton's reply to this sally was even better than his answer to King William, for, speaking in his broadest Cumbrian, he added, "Mebby sae; but what can yan say to sic like fowk?"

John Dalton was slightly above the average height; his frame, robust and muscular, was typical of his bucolic northern Cumberland blood. He enjoyed throughout life strong health, derived from good digestion and a firm pulse; he said he did not know what serious illness was. Even his studious and sedentary habits did not diminish his physical powers. In middle life, and without an hour's preparation, he would start on his mountaineering expeditions, and, carrying his instruments on his back, he climbed the heights like a mountain guide — so quickly, indeed, that one of his companions remarked, "Why, John, what are thy legs made of?" But his physical strength was not so much due to muscle as to brain. His cerebral development was powerful; his head, as testified by his broad-brimmed hat, now preserved in the museum of the Manchester Society, was brachycephalic; and the brain weighed $3\frac{1}{4}$ lbs. Many of his friends were impressed by the striking likeness of his broad expansive head to the well-known portrait of Newton, and Mr. Woolley, who placed a cast of Newton's head after death beside Dalton's after his decease, remarked that the likeness observed during lifetime became more remarkable in death. This similarity between the features of the

two great Englishmen is borne out by the following anecdote told by Mr. Ransome, the surgeon: —

"One evening I called on our venerable friend, whom I found seated, as usual, with his cat on his knee and his newspaper in hand. After the usual salutation, I espied, on a table close to my chair, a plaster mask, which I took up and forthwith rallied him upon overcoming his prejudices, and submission to the process, adding that I was rejoiced to find that he would leave us an accurate copy of his features. 'No,' he said; 'it is not intended for me.' I replied that if not a cast it must be an admirable model, intended for him. Again 'No.' Then, calling to mind the supposed similarity, I remarked, 'Then it must be either Newton or yourself.' During this period of my uncertainty he was chuckling and much amused, and then replied, 'Yes; it is a cast of Newton's face, which some friend has kindly sent me as a present.' I can only assure you that this remark of mine was honest and *bonâ fide*, the impression of the moment, and unconnected with any attempt to flatter him."

The description of Dalton's person, given by Dr. Lonsdale, is so graphic that I need not apologise for quoting it: —

"The massive contour of Dalton's head impresses you with the stamp of intellectual power and a capacity for the highest of human efforts. His prominent eyebrows shaded the deep-set eyes of quick discernment, whilst the use of large spectacles added to his general philosophic seeming and force of character. In his marked nose, rather massive jaws, and firm, deep chin, you saw the sturdy race of the 'north countrie,' not altogether free from an air of severity at times; these, however, were somewhat toned down by lips less masculine than

usual, and a physiognomy that offered blandness as well as firmness and penetration."

The portrait forming the frontispiece to this volume is a reduction from the well-known engraving of the painting by Allen in the possession of the Manchester Society. It gives an excellent representation of Dalton in the full vigour of life.

Dalton always dressed in the habit of a Quaker — knee-breeches, grey stockings and buckled shoes. He was careful of his personal appearance, dressed in good broadcloth, with spotless white neckcloth, and usually carried a handsome gold-tipped walking-cane. Although in his letters to "Friends" he used the "thee" and "thou," he did not do so always in conversation, nor in many ways was he so strict as the older members of the "Society": thus when in Paris he went to a representation at the Opera Comique. Reserved in general society, his conversational powers were seen at their best when surrounded by a few congenial spirits, enjoying a pipe of tobacco. Then he felt at home, and would amuse or instruct his fellows with a humorous story or with a philosophical disquisition.

On the whole, however, he was a silent man, and he could sometimes be rough and even rude in his manners. Thus one evening, when a few intimate friends were drinking tea with Dalton and the Johns family, some distinguished *savants* of the French Academy were introduced. They had come from

Paris to see the celebrated man. Dalton was somewhat put out by this incursion, and after the introduction said barely a word to the loquacious Frenchmen. Tea being over, Dalton quietly got up from table, called the old servant to bring him his lantern, and at once retired to his laboratory across the street, without taking further notice of the "deputation." Mr. Johns then asked one of the visitors what he thought of the philosopher. "Ah," said he, "Monsieur Dalton a une simplicité admirable." Still he was gratified by attentions thus shown him, and especially so when the distinguished visitors were his own countrymen, such as Thomas Thomson, Brewster, Leslie, or Chalmers, who frequently saw him. Perhaps he could not make himself readily understood by foreigners, they being unable — as doubtless was the case — to speak his mother tongue. He enjoyed taking the chosen spirits to his laboratory and explaining to appreciative ears what he was about, and showing them his home-made, simple, but generally effective apparatus.

Dalton never took part in any social, much less any political, matters; his interests and energies were wholly absorbed by his scientific work. Asked by someone why he did not take part in public affairs he replied, as he had done on the subject of marriage, "Oh, I never had time." But it is not to be supposed that he was churlish or selfish. That was not in his nature. He kept up to the last an interest in and an

affection for his old, humble Eaglesfield friends, and left at his death the greater portion of his property to his distant relatives.

Although undemonstrative, Dalton's friendships were firm and lasting. That he was generous is shown by the following anecdote, communicated to Dr. Henry by a lady who knew him well: —

> "A fire occurred in the works during my brother's minority. A few days afterwards, Mr. Dalton offered to my mother all the funds he had saved, if money was wanted. It was not required; but we thought it an act of very considerate kindness and friendship."

He was wanting, however, in the larger sympathies engendered by a liberal and academic education. As in his science, so in his social relations he worked in a narrow groove, and scarcely understood or appreciated action of a wider character. Self-reliant, and firm in his own opinion to the verge of obstinacy, he put little trust in the works of others. He discouraged the proposal of the Council of the Manchester Society to found what has now become one of the most complete libraries of periodical scientific literature in the kingdom by saying, "I could carry all the books I ever had on my head." This side of his character was certainly weak; had he done more to make himself acquainted with contemporary scientific literature, he might have avoided many mistakes into which he fell. But the human mind is what he might have called a complicated

"article," and who can say whether his precious qualities of independent and powerful thought might not have been stultified if he had more closely devoted himself, as less original minds are so apt to do, to the work of others.

From his upbringing Dalton was through life thrifty, if not parsimonious, in all that related to himself, though liberal and even generous to others. I have turned over many of his account-books from his earliest to almost his last days. Every little item of outgoing was recorded with scrupulous care. Before me, as I write, lies the last entry in his "Book of Accounts." It was—I pause to say "written," for the marks can scarcely be called writing—on July 15th, 1844. On the 20th of May he had had an apoplectic seizure, and on the 27th of July he was found dead in his bed.

This almost illegible scrawl, compared with the firm hand seen in the facsimile of a letter written in 1833 to Miss Johns, and found at the end of this volume, shows what a fall was there. Indeed, the beginning of the end came long before. On April 18th, 1837, his health was suddenly broken by a paralytic attack. And I find the following entry in his laboratory book for that year written in his firm hand, dated April 17th:—

"Thermomr. within doors in Society's room stands at 44° when a fire in the room."

Then, below this, the following, in much feebler lines:—

"This is the last memorandum before the illness; it was taken on the 18th. This new memorandum is taken the 2nd Nov., 1837."

Still he goes on with his laboratory work. The twelfth and the last volume of his notebook, in the possession of the Manchester Society, goes up to July, 1839, and is crammed with the results of experimental work, chiefly on the phosphates and arseniates. In 1840 he sent to the Royal Society a paper on the above subject. In it he re-asserted his old ideas on the composition of these salts, which were altogether at variance with modern views, and palpably erroneous. The Council of the Society, in refusing to publish this communication, were actuated by a proper regard for Dalton's lasting reputation. It was obscurely written, in parts hardly intelligible, and showed unmistakable signs of a mind diseased. Naturally, he was greatly mortified by this decision, and printed the memoir in separate form, with the indignant remark, "Cavendish, Davy, Wollaston, and Gilbert are no more," and, in concluding adds, "I intend to print my essays in future, to be appended to my other publications. Some of them are materially affecting the atomic system."

Although kindness itself to his pupils—all of whom spoke of him afterwards with affection and

respect almost amounting to reverence — Dalton did not relish being cross-questioned on scientific subjects by a tyro or by inquisitive persons who desired to get information out of him. To such he used to reply, "I have written a book on that subject, and if thou wishest to inform thyself about the matter, thou canst buy my book for 7s."

A characteristic anecdote is thus told by Dr. — now Lord — Playfair, who in 1844, the year of Dalton's death, was residing in Manchester: —

"I gave Dalton a small pamphlet, which was a reprint of two lectures which I had given to the Royal Agricultural Society. A few days after, Dalton sent for Peter Clare to ask him if he knew what I had given him, for he had mislaid it. Peter Clare assured him that he had received nothing from me, so the housekeeper was sent for, and she recollected that he had brought home a book with my name on it, but she was unable to say what it was. Accordingly, Peter Clare was sent to my laboratory as an ambassador, to ascertain what the book was and what was its value.

"As an author, I found some difficulty in estimating the value of the performance, but was soon relieved by being told that it was not the scientific but the money value which Dalton desired to know. I stated that the price, if sold, might be sixpence or a shilling. In about an hour Peter Clare returned with a message from Dalton that he had intended to present me with all his works; but as their cost was above thirty shillings, he could not think of giving them for a one-shilling present. He made, however, the entertaining proposal that I should gather together all I had ever written, send them to him with a priced list, and then he would make up his mind as to whether he could present me with his works. You may suppose that I was too much amused with the pro-

posal to hesitate, so I gathered together what I could, including my translation of Liebig's Agricultural Chemistry. After this had been done, I found that the declared value of my present was seventeen shillings less than that offered by Dalton; but, nevertheless, I boldly sent it in. Next day came Peter Clare with all Dalton's works, every volume having on its blank page, in the author's autograph, 'John Dalton, D.C.L., F.R.S., to Dr. Lyon Playfair, Jan., 1844.' But I was requested forthwith to hand over the 17s., which I immediately did, delighted to have the works from the great author, and prizing them the more from the droll characteristic way in which they came into my possession." *

This occurrence, we should remember, took place only a few months before his death, in the days of his decay, when the strong brain had become partially disintegrated, and when his notebook showed that painful indications of mental aberration had set in. For, in his better days, although he was frugal, he was always liberal.

That Dalton himself keenly felt this diminution in his powers is clear from a note left by Miss Johns. One evening, in 1840, speaking of his election as Foreign Associate, he referred to Laplace as the greatest man of the age, and of another French *savant*

* Amongst the Dalton papers, I found a memorandum about this "transaction" in Peter Clare's handwriting. On one side is written:—"John Dalton, D.C.L., F.R.S., &c. &c., to Dr. Lyon Playfair, Jan. 30, 1844." And on the other side:—

"Meteorology	£0 6 0
Chemistry, first part	0 7 0
Do., second part	0 10 6
Do., Vol. II., Part I.	0 12 0
	£1 15 6."

he said, "Aye, he was a nonentity, as I am now." "No, no," replied his friend; "you are not a nonentity yet." But he felt that he was speaking the truth; and there is little doubt that his persistence in his daily laboratory work tended materially to assist the deterioration of brain from which he was suffering. He likewise complained to various correspondents of his failing powers — his memory as altogether gone, and his faculties impaired. To Murchison he writes, "I succeed in doing chemical experiments, taking about three or four times the usual time, and I am long in calculation."

Apart from his laboratory, his chief enjoyment was the summer visits which he invariably paid to the lakes. After climbing Helvellyn, measuring its height, observing the temperature, registering the amount of atmospheric moisture by the dew-point, collecting samples of air for subsequent analysis, of which his notebooks contain many a score, he would return to the small hostelry at Wythburn, and after partaking of the simple but toothsome fare of the district — fresh-laid eggs, home-cured ham, crisp oatcake, Cumberland butter, and, last but not least, a draught of good home-brew — the philosopher would sit outside and smoke the pipe of contentment and peace, looking, with the quiet satisfaction which accomplished toil affords, at the pleasing prospects of fell and beck which the sunny landscape offered in such profusion.

In addition to these excursions Dalton, up at least to middle life, was a regular attendant at the "Quarterly" and "Great" meetings of the "Society." In his letters to his brother Jonathan he describes his travels by coach to Ackworth, to Warrington, and to Liverpool, as well as to the Metropolis. Often he acted as chaperon to some of the young ladies of the Society, who, it appears, preferred to have an unmarried rather than a married friend on such occasions. The following hitherto unpublished letter, written in early days, gives a vivid and amusing account of life in Liverpool a century ago, and exhibits Dalton's powers of quick observation. Nothing seems to have escaped him.

"MANCHESTER, 1 mo., 16th, '97.

"DEAR BROTHER,—I got well to Lancaster, staid all night, and took the coach in the morn for Liverpool, where I arrived between eight and nine; slept at George Binns', went to meeting next morning and was introduced to Wm. Rathbone, who invited me to sit by him. At the conclusion he wished me to go home with him into the country to Greenbank (about three miles), and he would go with me next morning and take breakfast with Mr. Yates, and in the meantime would transmit him a note to that purpose. I agreed, and Wm., his wife, and myself, all rode home in his whiskey. We had a good deal of conversation in the afternoon, and I was highly pleased with his candour, liberality, and good sense, as well as with his amiable wife. Notwithstanding there were seven strangers there besides myself, I was complimented with their most elegant bedroom, a splendid carpet, fire, etc. etc. Next morn we went about a mile across the country to Mr. Yates, to breakfast, where I found Wm.

Marshall, our classical tutor, James Wild and J. A. Yates, the son, both students. After breakfast we had some able discussions on points of religion and morality. Wm. Rathbone rode to town, and soon after Mr. Yates, Marshall, and I went also in Yates' single-horse chaise. Marshall and I made a few calls; we visited Betsy Benyon's school (in the meeting-house), which is an airy, elegant, and commodious one, and afterwards returned to the Park to dinner. Two former students came from town to spend the even with us. Next morn (third day) Mr. Yates and I rode to town. Just as we we were descending the hill we saw a boat start with full sail; I remarked to him I never saw any vessel sail so fast—we concluded it to be the Packet-boat. We had not been an hour in town before word came it had been overset, and nearly twenty people drowned. It was taking the Manchester Volunteers to the tender on the river. We left after two, and rode to Wm. Rathbone's at Greenbank to dine, where were Yates' family and visitants, Mr. and Mrs. Roscoe, etc. etc. (The etiquette in Liverpool is to sit down to dinner a little after three; as soon as the cloth is drawn, in winter, candles are brought in; when the bottle has gone round about half-an-hour, the ladies retire, the gentlemen remain till a servant informs them tea is ready, when all meet together again in the tea-room, where they remain till they are informed supper is upon the table.) Mr. Roscoe is a gentleman of the law, lately well known in the literary world for his celebrated 'Life of Lorenzo de Medicis'; he is also a poet, and man of taste for the fine arts. We dined and drank tea there, a very agreeable company, and went home in the evening. Next morn we were to have gone six miles into the country to breakfast with Nicholas Ashton, Esq., brother of Mrs. Yates, a gentleman of great fortune, who comes to town every morning in a splendid coach, and who has a son with us. Mr. Yates ordered three horses and the carriage to be in readiness for five of us, and they referred the decision of the question 'to go or not to go' entirely to myself. As the roads were indifferent and we were to be back by two I decided in the negative. J. A. Yates and I walked to town (two

miles), made a few calls, and returned by two, when we found a large party of young gentlemen. Soon after two coaches from Liverpool with ladies, mostly young, came up; we sat down to dine upwards of twenty. One of the young ladies was very handsome and accomplished; another of them was a most exquisite player; she gave us a specimen after tea. Some of the party staid supper. Next morn (fifth day) I walked to town and attended meeting, then spent an hour at James Kenyon's with Betsy and Jane; afterwards repaired to a place of rendezvous for our party, who had fixed to go and see the French prisoners. We obtained an order from the Mayor and went, seven in company. In going we saw a ship off, firing a *feu de joye;* I counted six seconds from seeing the flash to hearing the report, and was enabled to give the signal of the report at the exact moment for eight or ten times; it was a pleasing experiment. The prison is a noisy, turbulent place; there were between eight and nine hundred Frenchmen; we conversed with them in their own language, bought a few trinkets, and returned. Went to dine with Mrs. Case, of Duke St., a widow, whose sons were formerly with us. The lady keeps her coach and lives in high style; she is Mrs. Ashton and Mrs. Yates' sister. After tea she gave us a specimen of her performance upon the *grand pianoforte,* an instrument somewhat like the harpsichord, but much superior; it cost seventy guineas. At eight four of us went to spend the even with Mrs. Rogers, another widow, whose late husband's two sons were with us. A most amiable and worthy woman, and about a year and a half ago, when I first saw her, I though her the happiest; but the loss of an affectionate husband in the circumstances of a young and mixed family was like to be felt. We had some instructive conversation, and she favoured us with some excellent performance upon the harpsichord. We left about half-past eleven, and walked home that night. Next morn we had Mr. Nicholson, who is esteemed one of the first-rate flute-players in the kingdom, to breakfast. He gave us some exquisite airs, and furnished me on paper with many facts relative to sound. We had a party of

gentlemen to dine, chiefly literary—namely, Roscoe, Rathbone, Earle, Clarke, etc.; various desquisitions were upon the carpet. They staid tea, as usual. Next morn (seventh day) I took leave of them at the Park, and spent a day amongst my friends in town. Called upon Mr. Roscoe, and was shewn his elegant library, prints, etc. Visited John Harrison's, Ed. Saul's, spent the eve at James Kenyon's, where David Hodgson and Wm. Flounders favoured us with their company; slept at G. Binns'. Took the coach next morn at eight for Warrington, arrived after eleven; dined with George Crosfield, attended afternoon meeting, visited our worthy friend Samuel Evelyn—who was there; drank tea and supped at John Johnson's with Ellen Eaton, spent the rest of the evening at John Whitwell's, slept at George Crosfield's. Next morning made a few calls, and at three p.m. took coach and arrived safe at Manchester about six. It will be unnecessary to add the visit was highly pleasant, the respectability of three principal characters, Yates, Rathbone, and Roscoe, was made more particularly evident to me from the more private interviews I had with them, and it was enhanced to me by their numerous civilities. The stilish manner of living, however, I am not in love with. Breakfasting at nine, getting little till after three, and then eating and drinking almost incessantly to ten, without going out further than to the door, does not suit my constitution.

"Joseph Rooke's child got well again, and they are removed to a good shop in Deansgate, one of the principal streets, where they sell butter, bacon, candles, etc., and it is likely to do very well for them.—I remain, with love to mother, thy affte. brother,

"JOHN DALTON.

"P.S.—I have put in three profiles, but they are good for little, not having time. Spent this even (fourth day) with Wm. Fothergill (Simon's son), who is just arrived from America after one of the most perilous voyages I ever heard of; they were thirty-two days without having their clothes off, were sometimes washed out of bed by the waves breaking into the cabin, etc."

Honours, both from his own country and from abroad, poured in upon Dalton in later life. Davy's death had caused a vacancy amongst the seats of the eight foreign associates of the French Academy, and to this John Dalton was elected in 1830 — one of the highest honours which a man of science can receive. Other foreign academies — notably those of Berlin, Moscow, and Munich — placed his name on their roll of foreign members. He took part in the early meetings of the British Association at York, Oxford, and Cambridge. At the Oxford gathering, in 1832, the degree of D.C.L. was conferred upon him. Probably that ancient university never distributed its honours to four more distinguished men than on that occasion. They were John Dalton, Michael Faraday, David Brewster, and Robert Brown. Dalton evidently seemed proud of his scarlet gown; nor did this colour strike him as anything out of keeping with his Quaker habit, for to him both that and the green of the Oxford gardens appeared as a dull drab.

As early as 1829 Charles Babbage, of calculating-machine renown, started the proposal to obtain a Civil Service pension for the Manchester philosopher, and he interested Dalton's lifelong friend, William Henry (Magnesia Henry), in the application, who wrote a statement of the case, which, from his intimate personal acquaintance with and generous admiration of Dalton, gives

a just and true estimate of his character and labours: —

"Mr. Dalton never had, nor was ever given to expect, any reward or encouragement whatsoever from Government; and, having been in habits of unreserved communication with him for more than thirty years, I can safely aver that it never occurred to him to seek it. He has looked for his reward to purer and nobler sources: to a love of science for its own sake; to the tranquil enjoyment derived from the exercise of his faculties in the way most congenial to his tastes and habits; and to the occasional gleams of more lively pleasures which have broken in upon his mind when led to the discovery of new facts or the deduction of important general laws. By the moderation of his wants and the habitual control over his desires, he has been preserved from worldly disappointments; and, by the calmness of his temper and the liberality of his views, he has escaped those irritations that too often beset men who are over-anxious for the possession of fame, and are impatient to grasp prematurely the benefits of its award. For a long series of years he bore neglect, and sometimes even contumely, with the dignity of a philosopher, who, though free from anything like vanity or arrogance, yet knows his own strength, estimates correctly his own achievements, and leaves to the world — generally, although sometimes slowly, just — the final adjudication of his fame. Among the numerous honours that have since been conferred upon him by the best judges of scientific merit, in this and other countries, not one has been sought by him. They have been, without exception, spontaneous offerings, prompted by a warm and generous approbation of his philosophical labours, and by the desire to cheer him onward in the same prosperous career. Deeply as he felt these distinctions, they have never carried him beyond that sober and well-regulated love of reputation which exists in the purest minds, and is one of the noblest principles of action.

"In perfect consistency with Mr. Dalton's intellectual qualities are the moral features of his character, the disinterestedness, the independence, the truthfulness, and the integrity which through life have uniformly marked his conduct towards others. Nor is it on the Atomic Theory only that his reputation must rest; it has a broader basis in his beautiful and successful investigations into the subject of heat, into the relations of air and moisture to each other, and into a variety of other topics intimately connected with the stability and advancement of chemical philosophy. I therefore agree with you that Mr. Dalton has strong claims upon the national respect and gratitude, and contend for his title to reward, even though he may not have accomplished anything bearing strictly upon the improvement of those arts and manufactures which are the chief sources of our national wealth; for let it be remembered that every new truth in science has a natural and necessary tendency to furnish, if not immediately, yet at some future time, valuable rules in art.

"It would surely be unworthy of a great nation to be governed in rewarding or encouraging genius by the narrow principle of a strict barter of advantages. With respect to great poets and great historians, no such parsimony has ever been exercised; they have been rewarded, and justly, for the contributions they have cast into the treasury of our purely intellectual wealth. And do not justice and policy equally demand that a philosopher of the very highest rank, one who has limited his worldly views to little more than the supply of his natural wants, and has devoted for more than forty years the energies of his powerful mind to enlarging the dimensions of science, should be cherished and honoured by that country which receives by reflection the lustre of his earned fame? It is most desirable, then, that the British Government, by extending its justice to another not less illustrious, should be spared the deep reproach, which otherwise assuredly awaits it, of having treated with coldness and neglect one who has contributed so much to raise his country high among intellectual nations, and to exalt the philosophical glory of the age."

This excellent letter was brought under the notice of Lord Grey's Government. Lord Brougham professed himself to be favourable to the application, "but there were great difficulties"; and it was not till 1833 that anything was done, and then a sum of £150 a year was granted, as Lonsdale says, "by the richest nation on earth to the most gifted of persons and the chief scientific leader of his epoch."

There was at that time, it is clear, a violent popular outcry against pensions of all kinds. Lord Murray wrote: "This dislike approaches almost to fanaticism. It is the cry of the time, arising, no doubt, from former abuses; and while it subsists to such a degree it is hardly possible to obtain, for the most deserving, what the nation ought to feel a pride in bestowing." Still, thanks to the action of many influential friends, the thing was done, and the result communicated privately to Dalton by Mr. Poulett Thomson in the following correspondence: —

"Whitehall, June 22nd, 1833.

"Sir, — Although I have not the honour of enjoying your personal acquaintance, the gratitude which I feel for the distinguished services you have conferred upon science, as well as the respect which I entertain for your character, make me feel deeply anxious that some public mark of those feelings should be shown by the Government to which I belong, and induced me earnestly to press for the first opportunity being taken to offer such a testimony, however trifling the pecuniary amount of it might be. It is, therefore, with sincere pleasure that I inform you that his Majesty has been graciously pleased

to second their wishes in the manner which you will perceive from the accompanying note from Colonel Grey, which I have the honour to enclose. — I beg to subscribe myself, with the most sincere respect, sir, your faithful servant,

"C. POULETT THOMSON."

The enclosed note was: —

"DEAR THOMSON, — My father desires me to tell you that the King has been pleased to grant a pension of £150 nett to Mr. Dalton. — Yours truly,

"C. GREY."

Three years afterwards this grant was increased, the following pleasing letter — which I found amongst the Dalton papers — notifying the fact to Dalton: —

"TREASURY, June 6th, 1836.

"SIR, — I am happy to inform you that Lord Melbourne has had the satisfaction of recommending you to his Majesty for an increase of pension, and has desired me to inform you that your name has been placed on the pension list for an additional hundred pounds a year. I need not add that it gives me great pleasure to be able to communicate this intelligence to you, as I know no one to whom the country is more indebted for the advancement of science, or to whom such an acknowledgement is more justly due. — I have the honour to remain, sir, your most obedient servant,

"E. J. STANLEY.

"Dr. Dalton, etc. etc."

The fact that a royal pension had been granted to Dalton was first announced at the opening meeting of the British Association at Cambridge, when just

before the meeting Lord Monteagle asked the President, Professor Sedgwick, to refer to it in his address. The impromptu words in which the gifted geologist made mention of this are remarkable as expressing, in felicitous and feeling language, the debt which science owed to Dalton, and picturing his character in a manner both truthful and appreciative: —

"They had all read a highly poetical passage of a sacred prophet, expressed in language to the beauty of which he had never before been so forcibly awakened as at that moment: 'How beautiful upon the mountains are the feet of him that bringeth good tidings.' If he might dare to make an adaptation of words so sacred, he would say that he felt himself in the position here contemplated — of one who had the delightful privilege of announcing good tidings; for it was his happiness to proclaim to them what would rejoice the heart of every true lover of science. There was a philosopher sitting among them whose hair was blanched by time, whose features had some of the lines of approaching old age, but possessing an intellect still in its healthiest vigour — a man whose whole life had been devoted to the cause of truth. He meant his valuable friend Dr. Dalton. Without any powerful apparatus for making philosophical experiments — with an apparatus, indeed, many of them might almost think contemptible — and with very limited external means for employing his great natural powers, he had gone straight forward in his distinguished course and obtained for himself, in those branches of knowledge which he had cultivated, a name not perhaps equalled by that of any other living philosopher of the world. From the hour he came from his mother's womb the God of nature had laid His hand upon his head and had ordained him for the ministration of high philosophy. But his natural talents, great as they were, and his almost intuitive skill in tracing the relations of material phenomena,

would have been of comparatively little value to himself and society had there not been superadded to them a beautiful moral simplicity and singleness of heart which made him go on steadily in the way he saw before him, without turning to the right hand or the left, and taught him to do homage to no authority before that of truth. Fixing his eye on the highest views of science, his experiments had never an insulated character, but were always made as contributions towards some important end — were among the steps towards some lofty generalisation. And with a most happy prescience of the points towards which the rays of scattered experiments were converging, he had more than once seen light while to other eyes all was yet in darkness; out of seeming confusion had elicited order, and had thus reached the high distinction of becoming one of the greatest legislators of chemical science.

"While travelling among the highest mountains of Cumberland, and scarifying the face of nature with his hammer, he (the President) had first the happiness of being admitted to the friendship of this great and good man, who was at that time employed day by day in soaring among the heavens and bringing the turbulent elements themselves under his intellectual domination. He would not have dwelt so long on these topics had it not been his delightful privilege to announce for the first time (on the authority of a Minister of the Crown who sat near him) that his Majesty King William the Fourth, wishing to manifest his attachment to science, and his regard for a character like that of Dalton, had graciously conferred on him out of the funds of the Civil List a substantial mark of his royal favour."

"The announcement was received with prolonged applause."

The imagination may picture, if it can, the feelings of the son of the poor Eaglesfield handloom weaver as he sat in the Senate House of the University of Cambridge listening to this eulogium — the observed of all

observers — amidst the crowd of the most distinguished men of science of the nation. Not that Dalton would have felt in all this more than a just satisfaction. He knew full well how much more remains to be done than he, or any other single man, could ever accomplish, and this thought must have moderated and modified any feeling of undue pride which might under these circumstances not unnaturally have arisen. For he took a modest, and yet sensible, view of his position. He knew his own powers and defended his own views, but without conceit or self-laudation. Speaking once to a young man, he said: "Thou seems to have better talents than I possessed at thy age; but thou may want the thing I had a good share of — perseverance." If this is not everything, without it there can be nothing. Perseverance made him the best scholar in Pardshaw School; it enabled him to make 200,000 meteorological observations; it rendered it possible for him to establish the Atomic Theory; and it gained for him the highest recognition which a man of science can attain — a seat as one of the eight foreign associates of the French Academy.

In 1842 the British Association met for the first time in Manchester. That accomplished nobleman, Lord Francis Egerton, was President, Dalton's health being then so infirm that it was impossible for him to fulfil, as otherwise he would have naturally done, the duties of the chair. Lord Francis alluded to

this state of things in his introductory remarks as follows:—

"Manchester has, in my opinion, a claim of equal interest as the birthplace,* and still the residence and scene of the labours, of one whose name is uttered with respect wherever science is cultivated, who is here to-night to enjoy the honours due to a long career of persevering devotion to knowledge, and to receive, if he will condescend to do so, from myself the expression of my own deep personal regret that increase of years, which to him, up to this hour, has been but increase of wisdom, should have rendered him, in respect of mere bodily strength, unable to fill on this occasion an office which, in his case, would have received more honour than it could confer. I do regret that any cause should have prevented the present meeting, in his native town, from being associated with the name of Dalton as its president. The Council well know my views and wishes in this matter, and that, could my services have been available, I would gladly have served as a doorkeeper in any house where the father of science in Manchester was enjoying his just pre-eminence."

Whilst the Government and the nation were thus testifying to the value of Dalton's work, Manchester was not unmindful that she had a prophet within her walls, and his friends and admirers far and wide were induced to take steps to secure a suitable and lasting memorial of the great citizen in the town where he had so long laboured. The question as to whether

* Owing to Dalton's long residence of fifty years, Manchester, in the eyes of a vast majority of people, was considered his birthplace.

this should take the shape of a building devoted to scientific research, or of a more personal memorial such as a statue, was at length decided in favour of the second alternative. Dr. William Henry's opinion, expressed in the following words, settled the question: —

"I trust that the Committee will pause before they determine to abandon the proposition of a statue, and will decide on handing down to distant posterity the *viva ac vera effigies* of a man who will be honoured in all future ages, so long as science shall be known and respected. It will be a bequest which all future philosophers, as well as the world at large, will know how to appreciate. It will gratify the desire inherent in all men to call up distinct conceptions of the visible form and features which have been associated with intellectual endowments of the highest order. How such resemblances are prized will appear from the following extract of a letter recently addressed by Berzelius (one of the first of living philosophers) to a friend (Dr. Traill) who had sent him the portrait of Dalton: 'Mille et mille remercîmens pour ce cadeau. Je suis bien aise d'avoir une idée de la figure d'un homme dont une pensée heureuse a été si fertile en résultats scientifiques.' "

Soon afterwards the Memorial Committee reported as follows: —

"The Committee appointed to take measures for obtaining a statue of Dr. Dalton have great pleasure in reporting that the object is now in a fair way to be accomplished. Finding the proposal to be warmly seconded by the general view of the inhabitants of Manchester and the surrounding districts, and to be supported by willing and liberal contributors,* it appeared to them to be time to

* Foremost was a liberal subscription from Faraday.

trance of our magnificent Town Hall, and opposite to it is the fine statue of Joule, Dalton's great pupil and follower. A replica in bronze stands in the Infirmary Square, and has as its companions statues of Peel, James Watt, and Wellington. Thus is Dalton's memory kept green amongst the citizens of Manchester.

But this is not all that Manchester has done to honour Dalton. On January 26th, 1853, a towns-meeting was held for the purpose of founding a scholarship for the encouragement of original research in chemistry, to be held in the then newly-established Owens College, now the headquarters of the Victoria University. A sum of £4,000 was raised by public subscription, and a more fitting testimonial could not have been proposed. The establishment of a scholarship for scientific research was at that time a circumstance without a parallel; but, in spite of the novelty of the experiment, the experience of forty years has fully borne out the wisdom of the course which its originators adopted, and to-day a long list exists of "Dalton Scholars" who have contributed to the progress of chemical science, and many of whom hold high and responsible positions in scientific, manufacturing, and official life.

As has been stated, Dalton died on July 27th, 1844. Dr. George Wilson gives the following description of his last hours:—

"On Friday, July 26th, he returned to his room about a quarter after nine, and, going to his desk, on which

Atomic Symbols

BY

John Dalton, D.C.L. F.R.S. &c. &c.

EXPLANATORY OF A LECTURE

given by him

to the Members of the

Manchester Mechanics Institution

19th. October, 1835.

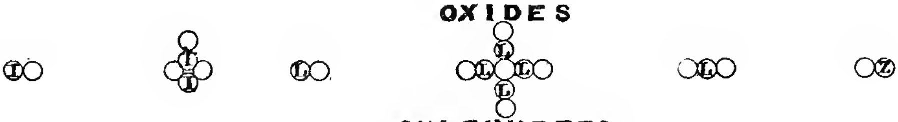
ELEMENTS.

Hydrogen .... *Oxygen* .... *Azote* .... *Chlorine* ....

Carbon .... *Phosphorus* .... *Sulphur* .... *Lead* ....

Zinc .... *Iron* .... *Tin* .... *Copper* ....

OXIDES



SULPHURETS.

COMPOUNDS.

Binary

Water ....

Nitrous gas ....

Carbonic oxide ....

Sulphuretted hydrogen ....

Phosphuretted hydrogen ....

Olefiant gas ....

Cyanogen ....

Ternary.

Deutoxide of Hydrogen ....

Sulphurous acid ....

Acetic acid ....

Nitrous oxide ....

Carbonic acid ....

Phosphoric acid ....

Nitrous vapour ....

Carburetted hydrogen ....

Prussic acid 2

Bicarburetted hydrogen ....

Tan ....

Quaternary.

Sulphuric acid ....

Binolefiant gas ....

Pyroxylic spirit ....

Quinquenary.

Ammonia ....

Nitrous acid ....

Prussic acid 2

Sexenary.

Alcohol ....

Pyroacetic spirit ....

Septenary.

Nitric acid ....

Decenary.

Ether ....

were usually placed the books in which he recorded his meteorological observations, he entered therein the state of the barometer, thermometer, etc., at nine o'clock, and in the column for remarks added the words 'little rain,' denoting that but little had fallen during the day. His servant observed that his hand trembled more than he had ever seen it before, and that he could scarcely hold the pen. Indeed, the book exhibits, in its tremulous characters and blotted figures, striking proof of the rapid decay of the physical powers. But there was the same care and watchfulness as ever manifested in the last stroke of the pen, for, having written 'little rain this,' he now noticed that the sentence was incomplete, and added the word 'day.' This was the last word that was traced by his tremulous pen.

"Early in the morning his housekeeper found him in a state of insensibility, and before a medical man could be summoned Dalton expired, passing away without a struggle or a groan — imperceptibly as an infant sinks into sleep."

The feeling in Manchester on the death of Dalton was so deep that a public funeral was decided upon. This was the more remarkable from the fact that he was a Quaker, and that to "the Society" all such forms are not only unusual, but antagonistic. His remains were placed in the darkened Town Hall, and they were visited by no less than 40,000 persons. On August 12th the funeral took place. A procession of more than one hundred carriages accompanied the body to the Ardwick Cemetery, and many hundreds of persons went on foot to the grave. All the shops and warehouses on the route, and many others, were closed. And thus the body of the simple-minded "Friend" was brought to mother earth.

During his long and frugal life Dalton had accumulated a moderate fortune. A few years before his death he had inherited a small family property from his brother, and he then felt able for the first time to buy himself some silver spoons. In his will he bequeathed the sum of two thousand pounds "to found, endow, or support a professorship of chemistry at Oxford, for the advancement of that science, in which the Atomic Theory, as propounded by me, together with the subsequent discoveries and elucidations thereof, shall be introduced and explained." But in a codicil he revoked this bequest, in order to provide more largely for the family of his friend, the Rev. William Johns, who had sustained in his old age a heavy pecuniary loss.

THE END.

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The Century Science Series.

EDITED BY

Sir HENRY E. ROSCOE, D.C.L., F.R.S., M.P.

JOHN DALTON

AND THE RISE OF MODERN CHEMISTRY.

By Sir HENRY E. ROSCOE, F.R.S.

Ready shortly.

MAJOR RENNELL, F.R.S.,

AND THE RISE OF ENGLISH GEOGRAPHY.

By CLEMENTS R. MARKHAM, C.B., F.R.S., President of the Royal Geographical Society.

OTHER VOLUMES IN PREPARATION.
